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THE MULTIPLICITY OF THOUGHT*

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Many writers have distinguished two types of mental processes. One kind of thinking is conscious, straightforward, predictable, and rather pedestrian; the other is confused, rich, productive of novelty, emotionally charged, and generally outside of consciousness. It is suggested that the latter arises from a multiplicity of processes going on together, while the former represents a single sequence among the crowd. These concepts are clarified by showing that sequence and multiplicity arise as alternative modes of organizing computer programs for pattern recognition. Even in the computer, multiple processing exhibits a superior ability to deal with novel or irregular input, while sequential processing appears less wasteful, and better adapted to fully predictable situations.

The properties that have been said to distinguish primary and secondary process, autistic and realistic thinking, creativity and constraint, insightful and rote activity, and the like, are shown to follow from the multiplicity of thought. Relevant experimental findings are discussed.

I. HISTORICAL SURVEY

The psychology of thinking seems to breed dichotomies. Nearly everyone who has touched the subject has divided mental processes into two (or more) kinds: productive and blind, creative and constrained, intuitive and rational, autistic and realistic, and so on. These distinctions seem to overlap; they share a common core of meaning which is difficult to describe precisely. In this paper I shall propose yet another dichotomy, but one which can be rather sharply defined: the distinction between *multiple* and *sequential* processes. I hope to show that many of the phenomena which underlie the other classifications can be clarified by it. First, however, we must take a closer look at these formulations themselves.

Productive and Blind thinking. These terms are taken from Wertheimer's classical discussion (1945). Essentially the same distinction was made by Köhler (1927) emphasizing the terms 'insight' and 'trial-and-error', as well as by other Gestalt psychologists. Recently it has been stressed again in Bruner's (1960) discussion of the aims and problems of education, in a chapter entitled 'The Importance of Structure'. Insightful thinking is responsive to the real structure of the problem. The situation is viewed as a whole, simultaneously, rather than one isolated part at a time. The productive thought process (or the effective teacher) focuses on the central features—and on the gaps—that appear in this direct inspection.

In Wertheimer's example, schoolchildren can learn how to find the area of a parallelogram in two fundamentally different ways. Blindly, they can follow some sequence of steps prescribed by the teacher or carried over from previous learning. Productively, they may see that a parallelogram can be transformed into a rectangle merely by lifting a triangle from one end and placing it on the other. The area of the rectangle thus produced is also the area of the parallelogram. Such a solution is responsive to the essential nature of parallelograms. A student who has understood it will be able to solve many other problems which involve the same essential characteristic, while one who has blindly followed the formula is limited to the situation in which he learned it. Moreover, structural learning is more interesting and exciting than its opposite, and leads to far better retention. No one doubts that 'learning by understanding' is very different from 'learning by rote', and generally preferable to it.

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* Parts of this paper were presented at the Goddard Conference on Psychoanalytic Concepts and Education, 1961, and at a colloquium of the Harvard Center for Cognitive Studies.

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Creativity and Constraint. There is widespread interest in 'the creative process' (cf. Ghiselin, 1952), in the identification of creative talent, and the like. Creativity here means much more than structurally sound thinking in Wertheimer's sense. Indeed, it is often opposed to 'problem-solving'. It is not limited to abstract thought, but is usually meant to include the original activities of musicians, painters and poets, as well as of scientists. The defining psychological characteristic of creative activity is not the quality of the product (even bad work can be creative) nor its uniqueness in history, but a certain freedom from constraint in the process itself. Learning by rote, copying, simple induction or deduction, the stereotyped application of artistic formulas—none of these is creative; they lack the spontaneity and unpredictability that the term implies. Problem solving, no matter how elegant, always involves mostly a response to environmental demands, while creativity erupts more or less unconstrainedly from within the person himself.

It is generally agreed that the creative process is not a conscious one. Artists and scientists unite in reporting that their ideas (images, expressions, etc.) simply 'appear', more or less clearly, later to be elaborated consciously. '(Creative) production by a process of purely conscious calculation never seems to occur' (Ghiselin, 1952, p. 15). Many have argued, however, that a conscious period of preparation is necessary to initiate the 'incubation' process that occurs out of consciousness. Maslow (1957) has argued that creativity is intimately bound up with the Freudian 'primary process', and Kubie (1958) has stressed its relation to the preconscious; these points of view will be elaborated below.

Intuition and Reason. This distinction has probably always been with us. Some judgements and decisions seem to be 'rational', 'logical' in that the person who makes them can explain the basis of his judgement, to himself or to another person. Others—hunches, guesses, feelings—are of obscure origin. Intuition is quick, and often compelling; reason is plodding and pale. Some persons are said to be more intuitive than others; women perhaps more than men. Intuition plays a prominent part in interpersonal relations, in our judgements of other people and our behaviour towards them. Neither the therapist whose interpretations are appropriate, nor the actor gifted with a keen sense of timing seems to base his actions on rational deliberation. Hunches and intuitions play a role in many other situations as well; one may feel inexplicably certain that a particular line of attack will yield the solution to a problem, that there will be a change in the weather or a fluctuation in the stock market. Mental processes of this kind seem to be common wherever there are situations too complex for ready logical analysis.

Bruner discusses the difference between intuitive and 'analytic' thinking in detail: 'Analytic thinking characteristically proceeds a step at a time. Steps are explicit and usually can be adequately reported by the thinker to another individual. Such thinking proceeds with relatively full awareness of the information and operations involved. It may involve careful and deductive reasoning, often using mathematics or logic and an explicit plan of attack. Or it may involve a step-by-step process of induction and experiment.... Intuitive thinking characteristically does not advance in careful, well-planned steps. Indeed, it tends to involve manoeuvres based seemingly on an implicit perception of the total problem. The thinker arrives at an answer, which may be right or wrong, with little if any awareness of the process by which he reached it' (1960, pp. 57–8).

Intuition shares certain properties with the creative process. Tauber & Green (1959) consider them essentially identical, and group them together with dreams and subliminal perception as 'prelogical' experience, opposed to the 'logical' processes of reason. This point of view comes close to the positions of Freud and Kubie, discussed below.

Autistic and Realistic thinking. This distinction was made by Bleuler (1912). Autistic thinking is dominated by effects; realistic thinking is sensibly and coherently goal-directed. 'It (autism) mirrors the fulfilment of wishes and strivings, thinks away obstacles, conceives of impossibilities as possible, and of goals as attained' (p. 404). Bleuler found it characteristic of the thought of schizophrenic patients and of dreams, noting also that it sometimes occurred in 'states of great distraction' and in mythology. McKellar (1957) has expanded these parallels. He regards imaging, creative activity, and other relatively 'uninhibited' mental states as essentially involving autistic thought.

In normal persons, autistic thinking occurs primarily in situations where the constraints of reality are loosened, as in sleep or in daydreams. Thus, there is no need for the thought process to restrict itself to the possible, to realistic representations, to ordinary sequences of time and place, and so on. It is relatively free to follow where impulse leads. This interpretation requires us to assume that ordinary people have powerful 'unrealistic' impulses, with certain characteristic modes of expression. Thus, it merges with the Freudian conception of the primary process, on which several of the dichotomies we have discussed are essentially based.

Primary and Secondary process. In *The Interpretation of Dreams* (1900) Freud found it necessary to assume that two fundamentally distinct modes of mental functioning were available to every person. The 'primary' process is revealed by the 'absurdity' and 'incorrectness' of dream experiences. Any train of thought which has suffered repression, and has been drawn into the unconscious, will be governed by the primary process. Its characteristics include (1) condensation: the emotional loadings of various ideas pass freely from one to the other (along varied associative pathways), and the intensity of many may ultimately be concentrated in a single conceptual unit; (2) compromise: the formation and emotional stressing of new ideas intermediate among existing ones; (3) the use of very loose connexions (puns, clang associations, etc.) as associative channels; (4) toleration of contradictions: any thoughts whatever may co-exist or combine. The function of the primary thought process is to find discharge for emotions. It takes place in the unconscious; that is, among ideas which have been permanently repressed and cannot become conscious. For this reason, the emotions involved are always infantile urges. In Rapaport's (1951, p. 693) terms, the primary process is 'drive-organized'.

The mental activities of the new-born infant are assumed to be almost entirely governed by the primary process. Gradually, a 'secondary process' develops in which this sort of activity is inhibited in favour of reality oriented thinking. The delays and detours which are encountered in the search for emotional discharge result in the growth of rational and conceptual thinking. The secondary process is 'conceptually organized' (Rapaport, 1951, p. 696). Conscious and preconscious ideas are governed by the secondary process. Freud assumed that primary-process thought never became conscious without substantial secondary censorship, like that of the dream. This differentiates the concept of primary process from that of autistic thinking; the latter term can describe conscious as well as unconscious processes.

Many psychologists and others have speculated that the primary process is the source of creativity. It seems to have the unpredictability and freedom from constraint which the creative process requires. This view is supported by the fact that the creative act is inaccessible to consciousness. A common modification of this theory (e.g. Rapaport, 1951, p. 719; Maslow, 1957) holds that creative and intuitive thinking result from suitable co-operation between primary and secondary processes. The former supplies spontaneity, vigour, and effortless invention; the latter elaborates and re-orders the products to adapt them for artistic, communicative, or other purposes.

Unconscious, preconscious, and conscious. The primary process model has recently been challenged by Kubie in his provocative book on creativity and neurosis (1958). He rejects the simple dichotomy of the two processes because it assigns the same origins to creativity as to pathology: both are said to arise from the uncontrolled and uncontrollable unconscious. His own conviction is that creative activity is blocked by neurosis and released by psychotherapy. Nevertheless, he agrees that the source of creativity cannot lie in the systematic and communicable processes which Freud called 'secondary'. Thus, Kubie argues that a *tri*chotomy is needed to handle the facts: the communicable processes of consciousness, the quick and flexible thinking that results in creativity, and the deep unconscious restraints of anxiety and neurosis. The middle category he calls *preconscious*. This is a substantial extension of Freud's views. Freud thought of conscious and preconscious as similarly organized; the latter consisted simply of ideas that could become conscious (without censorship) but did not happen to be so at some given time.

Kubie shows, with many rich examples, that preconscious activity is fast, complex, and prolific. It has the functional properties that other theorists ascribe to the primary process, without being organized around either infantile drives or systematic conscious conceptions. However, it rarely operates independently of the other systems. 'Preconscious processes are assailed from both sides. From one side they are nagged and prodded into rigid distorted symbols by unconscious drives which are oriented away from reality and which consist of rigid compromise formations, lacking in fluid inventiveness. From the other side they are driven by literal conscious purpose, checked and corrected by conscious retrospective critique. The uniqueness of creativity, i.e. its capacity to find and put together something new, depends on the extent to which preconscious functions can operate freely between these two ubiquitous concurrent and oppressive prison wardens' (1958, p. 45).

II. SEQUENTIAL AND MULTIPLE PROCESSES IN COMPUTERS

The common core of all these theoretical dichotomies seems to be the distinction between a relatively well-ordered, easily describable, and efficiently adapted thought process on one side, and a simultaneous and superficially confused profusion of activity on the other. Apparently there are two different modes of handling the external and internalized information with which the mind must deal. It cannot be a coincidence that two corresponding modes have appeared as alternate possibilities in the design of 'artificially intelligent' systems—that is, in programming computers to perform quasi-intellectual tasks. In that field, the two possibilities are often called 'sequential' and 'parallel' (Selfridge & Neisser, 1960).

'Intelligent' activity in computing machines

How is it possible for processes in machines to teach us anything about human thinking? Human mental activity is more complex than any existing computer program by many orders of magnitude. The 'intelligence' that has been achieved artificially is either extremely specialized or almost trivial. Moreover, the human mind may—indeed, probably does—have resources and methods that are unimaginable in machines such as those in existence today. Clearly, there is an empirical question here. Useful analogies may exist, or they may not. It is for the psychologist to decide whether a particular model or theory has value for him, regardless of its origin. However, he can make such a decision only after expending the effort necessary to become acquainted with the model in question. For this reason, we must consider some of the problems involved in programming computers to behave sensibly.

The activity of a computer consists of information processing. What is meant by 'information' and by the unfortunate verb 'process' in this context? The computer has some input mechanism by which data can be made available to it. In existing machines, the data are typically holes in IBM cards, or alterations in magnetic tape. Whatever subject matter the computer is to deal with—chess positions, foreign language text, written characters, etc.—must first be coded into one of these formats, just as whatever a man deals with must take the form of visible light, audible sound, or some other perceptible form of energy. The configuration of holes in any given set of IBM cards is one among a great many possible configurations. This *selection* is the 'information' referred to: the computer will do one thing rather than another because the holes are arranged as they are. That is, it acts on the basis of the input information.

What the machine does depends on the program of instructions that has been pre-stored in it. The information that has been put in may be modified, or new information may be written. These actions may be made *conditional* on the input, or on intermediate results. 'If the input has characteristic A, add 1 to the contents of register X; if it has characteristic B, copy the first ten cards of input into registers R for later use; if C, execute the series of instructions beginning at Y; if neither A, B, nor C proceed to the instructions at Z.' Eventually, the program may call for the *output* (on more cards, or tape, or typewriter) of some part of the internal information.

What is put out will depend on what was put in and on the intermediate 'processing' that it has undergone. It is temptingly easy to give psychological names to different kinds of processing. Suppose the program takes one course of action if the input possesses a certain general property, and another if it does not: then this property has been 'abstracted'. Or suppose the program has some provision for modifying its own instruction sequence if certain intermediate results are obtained: it will handle further input differently and has 'learned'.

In fact, it is very difficult to write programs that will successfully 'abstract' interestng properties of the input. In computerman's language, this is called the problem of 'pattern recognition', and has received a good deal of attention (Minsky, 1961; Selfridge & Neisser, 1960). The difficulty arises partly because the precise informational basis of the properties we would like to abstract (of the patterns we would like to identify) is unknown, or variable, or both. Spoken words, or written letters, or pathological E.E.G. records, form classes of patterns that people can identify but for which the defining stimuli are hard to specify. We shall consider the recognition of letters in some detail, in order to illustrate the two modes of analysis referred to above. The input problem itself will be ignored; suppose that by some television-like process a photograph of any printed or written letter can be converted into an array of holes on punched cards and fed into a computer. What shall be done with this information to determine which of the twenty-six alphabetic characters it represents?

One possibility is to have a standard form of each letter stored away in advance, and compare the newcomer with each of these to determine the best match. For reasons discussed elsewhere (Selfridge & Neisser, 1960) this procedure is not advisable. It is better to base the decision on the presence of various subordinate features of the input: lines, curves, open spaces, and the like, If these can be identified, how shall information about them be used in deciding which letter the input represents? We shall consider two alternatives: sequential logic and parallel processing.

Sequential processes

Sequentially, one may proceed as follows: is there a vertical straight line? If so, the letter is B or D or E or...; if not, the letter is A or C or V, etc. The question divides the universe of possibilities into two categories. The next question depends on the answer to the first. If there is a vertical line, one might ask if there is also a right angle. This divides E, F, T, etc., from N, M, U, etc. A series of such questions is called a 'logic' or a 'decision tree', and can easily be devised to make an unequivocal decision about every input. Many programs for letter recognition have been written on this basis. They work rather well, *provided the input is restricted to printed characters in particular founts.*

If the computer is given *hand-printed* characters to identify, such a program will score almost at random. The critical defining properties simply are no longer critical. A hand-printed E might have neither a vertical line nor a right angle, while an A might have both. Almost every conceivable kind of 'distortion' arises in hand-printing. These variations are not disturbing to the human reader; people can identify about 96% of randomly selected hand-printed characters even without the aid of context (Neisser & Weene, 1960). Nevertheless, a sequential program is unable to cope with them. The reason is obvious: a process which goes consecutively from one decision to the next must go astray if even a single decision is wrong. Only where no uncertainty exists is such a program dependable. Error-correction procedures are possible, but they will become unmanageably complicated if the variability of the input is large. Such a program proceeds by rote, and like all rote procedures it is inadequate in the face of novelty.

Multiple processes

Evidently, we need a program that sees the letter 'as a whole'. How can this be accomplished? Essentially *by asking all the questions at once,* instead of letting each answer determine the next question. Suppose the program examines the input for many different properties simultaneously. Letters are ultimately identified by weighted averages of the results. Such features as vertical line and right angle are given weights which increase the probability that the program will decide T and decrease its chances of saying O. Even a great deal of variability need not lead the program astray, because letters are effectively defined by the totality of their features. Selfridge (1959) originally proposed that the identifying operations be called 'demons' and the entire program a 'pandemonium'. This term suggests more confusion than is actually involved. 'Parallel processing' is also misleading, since it suggests that the various operations are very similar and that they never meet; neither implication is desirable. I shall use the term 'multiple processing' for such systems.

A program which identifies hand-printed characters by multiple processing has been written and tested by Doyle (1960). It is very nearly as good at its job as people are, reaching an accuracy of about 90%. It uses twenty-eight rather arbitrarily selected operations, most of which count gaps, or strokes, or concavities in various directions. Its accuracy is not much diminished if one or another of its operations is eliminated. The operations are not literally simultaneous; that would be impossible within the physical limitations of present computers. They are functionally simultaneous, in the sense that all of them are completed before any decision is made.*

Multiple processing has several fundamental advantages over the sequential procedure. Not only is it effective in the face of ambiguous cues, but it displays a certain nonchalance about internal errors. In contrast, a single false step in a pre-arranged sequential program is fatal. Moreover, learning and adaptation are relatively easy to incorporate into a multiple process. A sequential program on the other hand is very difficult to improve. Suppose some sequence of operations has classified the input as A when it should have been R. Which operation went wrong? There is no obvious way to find out, and one cannot readily introduce experimental changes since any alteration of the sequence is likely to have major consequences. In a multiple process, on the other hand, small changes in the weights given to various features may produce improvement, and at least will not upset the system as a whole. Doyle's program went through a learning phase, to find the results of the twenty-eight operations applied to a sample of known letters; the operating phase then made use of combining weights based on these results. In other words, the programmer did not need to know the defining characteristics of hand-printed letters at all; he only provided an array of relevant operations for the computer's use.

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* The program will not be described further here, for fear of wearying the reader. Details are available in Doyle (1960) and in Selfridge & Neisser (1960).

It is important to stress that the various operations in a multiple program do not have to be independent of one another. They may be as redundant as desired, if computer space is available. On the other hand, they need not all be of one type, but can focus on very different kinds of features. Nor need these features be 'elementistic' in the sense protested by Gestalt Psychology. It is easy to imagine an operation concerned with the degree of circularity of a pattern, or with the extent to which it is symmetric. This raises another point. How can the computer establish whether a pattern is circular? Only by a set of subordinate operations, which themselves may be organized either sequentially, or, again, in multiple. Thus, a multiple program naturally involves hierarchies of operations. A hierarchy may extend in the other direction also. For example, the identification of a letter may play the role of a single operation in a complex whose intent is the identification of printed *words*. The superordinate stage again could be programmed sequentially (each successive letter as it was identified might narrow the subset of possible words) or in multiple (all the letters might be examined and their results combined into a best guess at the word). Such hierarchies almost certainly play a major role in human cognitive processes.

Multiple processing is not always the best way to program a pattern recognition problem. It involves a great deal of wasted 'effort' (computer time or space) because every operation is applied to every input. In any situation where a correct and decisive sequence of operations can be established, sequential programming will be very much more efficient. Only where such a procedure is impossible or unknown can multiple processing be justified. Moreover, an organized sequence has a certain clarity which is absent in the 'pandemonium' of a multiple process. At any given moment in a sequence one can (in principle) say just what the machine is doing, and what it will do next. When a multiple process is operating (assuming true simultaneity of operations) no such statement is possible. Different parts of a multiple machine may appear to be engaged in quite unrelated and even useless activities. This is no mere appearance, for in any given instance many of the operations are indeed useless. Nevertheless, it is just this apparently incoherent and chaotic profusion of activity which enables the program to make quasi-intelligent identifications of patterns in its environment.

III. IMPLICATIONS FOR A THEORY OF THINKING

My thesis is that human thinking is a multiple activity. Awake or asleep, a number of more or less independent trains of thought usually co-exist. Ordinarily, however, there is a 'main sequence' in progress, dealing with some particular material in step-by-step fashion. The main sequence corresponds to the ordinary course of consciousness. It may or may not be directly influenced by the other processes going on simultaneously. The concurrent operations are not conscious, because consciousness is intrinsically single: one is aware of *a* train of thought, but not of the details of several.

The main sequence usually has control of motor activity. Cases where it does not (where behaviour does not correspond to consciousness) impress the observer as bizarre or pathological.

The characteristics of primary- and secondary-process thought

Before we examine the analogy in detail, a word about the role of motivation in thinking is appropriate. Why are some operations performed rather than others? Why do some processes become a part of the main sequence while others do not? The most consistent view is that all thinking is motivated; the course of processing depends on both the enduring and the cyclical needs of the thinker. Various needs have various consequences. One fundamental need is for the maintenance of an orderly and adaptive sequence in one's own behaviour. For social as well as physical and physiological reasons, our *actions* have to display a certain degree of self-consistency. This demand is probably responsible for the original development of a 'main sequence' within the multiplicity of thought. Certainly it accounts for the extent to which the main sequence is inviolable while we are awake, and our consequent belief that men can think of only one thing at a time. (If the main sequence is *not* inviolable, if our actions are not governed by an orderly sequential process, the result must be either immobility or chaos, i.e. psychosis.)

The multiple operations can combine and influence one another in many ways. In the computer program for letter recognition discussed above, the processes which recognized various features were combined by a simple weighted sum. In human thinking, every sort of fusion, exclusion, disjunction, and so on may occur. The result of this facile interaction is the 'primary process' described by Freud, in which condensation, compromise, and disregard for logic are the rule. The 'secondary process' is the main sequence itself, proceeding sequentially through steps whose logic has been preprogrammed by realistic experience. Kubie's 'preconscious' consists of multiple processes, while the main sequential process is his 'conscious'. Indeed, both Freud and Kubie refer explicitly to simultaneous processing; e.g. 'It is really not easy to form an idea of the wealth of trains of unconscious thought striving for expression in our minds...' (Freud, 1900, p. 478).

Why is the primary process 'drive-organized'? What accounts for the 'autistic' (i.e. personally motivated) character of our multiple processes? Again, the answer is hardly original. All thinking is motivated, but personal motives show through more clearly in processes which do not have control of behaviour and thus are relatively unconstrained by realistic demands. 'Autistic Thinking' in Bleuler's sense occurs when the main sequence is more or less detached from motor activity and is affected by logically irrelevant processes going on at the same time. Its 'irrational' characteristics stem from multiplicity, while its motivational and affective properties arise because the course of processing is governed by personal needs. Multiple processing is also the source of symbolism and over-determination in this kind of thought. One sequence of processes may be analysing the shape of something seen (or remembered) while others work with features that have emotional significance; all of them may recombine to turn the main sequence in a novel direction.

The existence of more than one process implies that the main sequence may be altered in 'unpredictable' ways at almost any moment. The change will usually be in a direction away from immediate adaptation to the external world and toward an emphasis on inner needs. Because such changes are often maladaptive they may produce anxiety, and restraints on the recombination of processes are usually developed during childhood. As a result of these restrictions on the flow of information, much of multiple activity in adulthood may have no effect, or its influence may be limited to certain stereotyped 'symbolic' expressions. These limitations are Kubie's 'unconscious'. People differ widely in the extent to which they are free to use the multiplicity of their own thoughts.

Limits on the complexity of multiple thinking

I propose that not only autism, but also intuitive, creative, and productive thinking depend on the use of multiple sequences of mental activity. This brings up an important question: what limitations are there on the nature and complexity of processes off the main sequence? If activities outside of consciousness had only the infantile logic of Freud's primary process it would be hard to see how they could be responsible for intelligent activity. In our approach to this problem, the analogy to a physically realizable computer may be helpful again. There are three different kinds of restrictions that limit the diversity and complexity of processes in a machine. It will be instructive to see what form they would assume in human thinking.

First, a computer may simply be constructed in such a way that certain operations can only be conducted one at a time. For example, most computers have a single 'central processing unit' in which all arithmetic operations must be carried out. This is the reason why the operations in Doyle's letter-recognition program were actually performed one by one, although they were 'functionally' simultaneous. However, one can imagine systems that are not so restricted (e.g. Holland, 1959). Some elementary mental activities are limited in just this way: seeing, for example, requires visually specialized regions of the nervous system. I am assuming that the parts of the brain underlying the processes of human thought are free of this limitation. (It is worth noting that, anatomically, the human cerebrum appears to be the sort of diffuse system in which multiple processes would be at home. In this respect it differs from the nervous system of lower animals. Our hypothesis thus leads us to the radical suggestion that the critical difference between the thinking of humans and of lower animals lies not in the existence of consciousness but in the capacity for complex processes outside it.)

A second limit on the achievements of computers arises simply because no one may know how to program the task in question. Either it cannot be performed with the operations available, or the way to perform it has not yet been discovered. Corresponding restrictions certainly apply to thought processes. Insoluble problems exist. However, this is true whether the activity of the mind is multiple or sequential, and thus is irrelevant to the present argument.

The final limitation on computer performance is a critical one. Any given machine has a finite capacity. There is only a certain amount of 'space' in which instructions and information can be stored. More complex programs take up more space. If two programs are executed simultaneously, each has less space available than if it were being carried out alone, and is thus more limited in what it can do. There is no way for human thinking to escape this principle. If several processes occur together, none can be as complex as the subtlest possible single processes. This must lead to problems in the allocation of mental resources: shall the main sequence use all the facilities, and prevent multiple processing altogether? Shall off-sequence processes occur only at a low level of complexity? Are a number of equally complex activities permissible? Can an off-sequence process be more complex than the main sequence itself? Of course, this 'problem' is not 'solved' by a planner. Some distribution of resources actually occurs, as the evolutionary result of interactions among ongoing processes and with the environment. We can safely predict that: (1) there will be individual differences in the depth which multiple processing attains; (2) the existing allocation of resources will usually not be the most efficient possible.

We are assuming that the main sequence (conscious activity) is in close touch with the environment because it must direct behaviour. Its activity is therefore crucial for adaptation, and we may reasonably suppose that it is accustomed to the lion's share of capacity. This conclusion is borne out rather directly by some of Broadbent's (1958) material, as well as by the 'primitive' character of autistic or prelogical thought. However, there is no reason to believe that this is necessarily or universally the case. At times when the activity of the main sequence is not demanding—for example, during sleep or the performance of routine activity—other sequences, out of consciousness, may attain high degrees of complexity. As we have seen, such states of disengagement do seem to play a critical role in creative thinking. Thus, considerations of capacity explain the role of the 'incubation' phase of thought.

It is not always easy to establish the relative 'complexity' of different mental processes. The information capacity which a computer requires to perform a particular task depends not only on the problem itself, but also on the mechanisms with which the machine is equipped and on the ingenuity of the program. One of the main lines of progress in the computer field is the development of 'programming languages'—and, to some extent, of computers specially designed for them—which make previously intricate operations comparatively simple. We do not know what simplifying mechanisms, both innate and acquired, are available for thinking. Plausible speculation is possible: for example, it seems likely that people are especially efficient at handling two- and three-dimensional properties of the visual input. In other words, the same amount of information may require less capacity coded in terms of spatial relationships than in terms of temporal sequence. (Unger, 1958, has designed a computer which would have this property to some extent.) This assumption would explain the predominance of visual imagery in dreams, and perhaps also our preference for visual models and metaphors for thinking, from 'insight' to 'point of view'.

Thus far, we have considered multiple processes which were relatively independent of one another and somewhat free of realistic constraints. To such processes we have ascribed 'autistic', 'prelogical', and 'creative' thinking, in its various guises. The 'productive' thinking which produces insightful solutions of problems does not seem to be quite the same kind. The primary process will not readily find the area of a parallelogram. Such problems require a grasp of the relationships among various parts or, perhaps, among various subproblems. There are lines that slant, angles, a central rectangle, a notion of what 'area' means, and so on. No part alone is enough, nor is just any concatenation of parts. But if the analyses of the parts go on simultaneously, along related sequences, there may come a moment when a higher-order operation (in the parallelogram case perhaps some part of the spatial systems hypothesized above) can combine them adaptively. In this situation, 'insight' seems to involve building a novel hierarchy of existing processes. There is ample evidence already that the 'components' of insightful behaviour must be established in advance (e.g. Birch, 1954). The present suggestion is that their *potential* existence is not enough: they must be *simultaneously* operative if a novel recombination is to emerge.

IV. EMPIRICAL EVIDENCE

All the foregoing is speculative. The fundamental assumption on which my argument rests is that people commonly or constantly think about several things at once. Is this really plausible? Can it be substantiated? In a sense, the most important evidence has already been presented: the theory stands or falls by its success in integrating the facts about human thinking. Nevertheless, a problem arises because the assumption of multiplicity contradicts the common belief of both laymen and psychologists. There is a widespread conviction that it is impossible, or at least unusual, to do even two things at once. This conviction is based marginally on objective evidence, but primarily on introspection. Everybody has had the experience of trying to think about two things at the same time, and ending with confusion about both. These experiences are valid, but what they show is only that we cannot be *conscious* of two complex processes together. They give no testimony about processes outside of consciousness. Our subjective certainty of the singleness of thought does not contradict the present theory. Indeed, it is incorporated in the concept of the main sequence.

A different kind of subjective experience confirms the possibility of simultaneity. In what is called a 'double take' we first hear a statement one way, then realize with a start that something quite different was said. Two analyses of the auditory input are represented here. The first initiates a conscious train of thought, which is interrupted by the results of the second. Thus, the second, significant, interpretation must have proceeded outside of consciousness until its importance was 'understood' and a consequent alteration of the main sequence occurred. Something like this must happen on a very large scale in cases of multiple personality. In such cases, one of the personalities often is aware of what another personality is doing or thinking, and can make use of its knowledge later.

Experimental findings

Within experimental psychology, there have been a number of studies indicating the difficulty of multiple response or multiple conscious activity (Woodworth & Schlosberg, 1954, pp. 87–90; Broadbent, 1958, Chapter 2). Subjects cannot follow two conversations at once, or make two responses at once, without decrement in performance. We cannot review these studies in detail. However, most of them would have required their subjects to have two 'main sequences'. The theory put forward here predicts that two things can be done at once only if (*a*) they do not both require response; (*b*) they do not both have a high level of complexity; (*c*) at least one of them is 'automatic' enough to be outside of consciousness. Ordinary experiments on overlapping activities do not meet these criteria.

To obtain more direct evidence, we have been conducting experiments on the visual scanning of printed lists. Preliminary results have been reported elsewhere (Neisser, 1962). Subjects scan a list of 'words' (actually consonant strings) until they find one that has some critical property. The search time, divided by the (variable) number of words *ahead* of the critical item, is a measure of the average time used by the subject to determine that each intervening item does *not* have the critical property. This measure is uncomplicated by reaction factors, since the subject makes no response to these non-critical items. In the present context, it is of interest to compare time-per-word if the critical property is a single letter (say Z) with time-per-word if the subject is looking for either of two letters (say Z *or* Q). All of our experiments agree that with practised subjects time-per-word is no longer in the latter case than in the former. Indeed, results of a recent experiment (unpublished) suggest that it takes no longer to scan for any of *ten* letters than for one alone. These results establish that multiple processing can occur, at least at the perceptual level. The theory proposed here also predicts that reactions to multiple cues will be slower in infra-human species than in man, but we have not carried out relevant experiments.

It is not as easy to generate novel predictions about thinking itself, because of the number of variables on which the successful problem-solving or imaginative thinking depend. It has been shown above that the tendency for ideas to arise during sleep or routine activity follows from considerations of capacity. One further prediction is worth mentioning. It has become common to have subjects 'think aloud' during experiments on problem solving (e.g. Newell, Simon & Shaw, 1958). If the views expressed in this paper are valid, such a procedure will substantially change the nature and course of the thought process, by limiting it to the main sequence. It may be possible to test this prediction experimentally.

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BI-POLAR AND UNIDIRECTIONAL SCALES

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By comparison with physics, the dynamic aspects of psychological measurement have not received sufficient attention. In psychology there is a need for categories that are fundamentally bi-polar as well as for single unidirectional categories. The consequences are discussed (*a*) for the overall classification of scales, and (*b*) for the application of Guttman scaling techniques.

I. Introduction

In classifying scales as nominal, ordinal, interval or ratio, ought not investigators first to consider whether the scales are bi-polar or unidirectional? That they should appears to follow, even in the case of nominal and ordinal scales from the work of several investigators, e.g. Helson (1948) on adaptation level, Festinger (1957) on cognitive dissonance, and also from Osgood's semantic differential (Osgood, Suci & Tannenbaum (1957). To put the problem another way, are not bi-polar 'double-category' measures in which two subclasses are separated required in psychology, as well as unidirectional 'single-category' measures? If this requirement is admitted, it may be necessary to make a revision of Stevens's fourfold classification of scales (Stevens, 1951). Bi-polar nominal and ordinal scales would fail to fit neatly into any of Stevens's four main types, and it is sometimes difficult to distinguish between interval and ratio scales. Some instances in psychology of the need for double-category as well as for single-category measures, and the necessity in factor analysis to distinguish carefully between bi-polar and unidirectional factors, under certain circumstances, have already been pointed out (Thompson, 1961, 1962).

II. The classification of scales

The classification of scales into nominal, ordinal, interval and ratio, has been primarily the work of Stevens (1951, pp. 26–30). In a nominal scale, numerals are used merely as labels or type numbers, e.g. the 'numbering' of football players or the listing of consumer products. An ordinal scale arises from the operation of rank ordering and any 'order-preserving' transformation will leave the scale form invariant, but nothing is presumed about the size of the intervals between the ranks. According to Stevens 'With an interval scale we come to a form that is "quantitative" in the ordinary sense of the word' and the intervals between successive positions are regarded as equal. Here, he says 'almost all the usual statistical measures are applicable...unless they are the kinds that imply a knowledge of a "true" zero point. The zero point on an interval scale is a matter of convention or convenience, as is shown by the fact that the scale form remains invariant when a constant is added'. The Celsius and Fahrenheit scales of temperature are examples of interval scales. But with a ratio scale, according to Stevens, an absolute zero is always implied (e.g. the Kelvin scale of temperature), and the numerical values of a ratio scale can be transformed only by multiplying each by a constant.

Most psychologists would probably say that with nominal and ordinal scales, as they are not quantitative scales, questions of polarity or direction do not arise, and would agree with Stevens's fourfold classification, in terms of which interval scales, being expressed by both positive and negative numbers, are bi-polar by definition, while ratio scales, being expressed only in terms of positive numbers, are unidirectional. This position can however be challenged. With nominal scales the situation is not as simple as it appears, and it can be argued that words themselves involve opposition, and consequently that some nominal scales are implicitly bi-polar. Linguistic psychologists such as Carroll (1955) and Osgood have shown a persistent tendency to work with opposites, and to use bi-polar pairs of words such as *good–bad, strong–weak, vigorous–placid, ordered–chaotic, complex–simple*, etc., rather than single words (see e.g. Sebeok, 1960, pp. 286–7). Using the technique of the semantic differential Osgood *et al.* (1957) have employed large numbers of word pairs as bi-polar rating scales for concepts and objects, and have over a long period derived the same basic factors from such material; they have carried out experiments which go a considerable way towards confirming the reality of semantic opposition as a universal language phenomenon (see especially pp. 146–52, 158–60 and 327–8). It may well be also, that speech itself is possible only through the opposition of basic sound forms (Cherry, 1957). Outside the area of language Helson's (1948) work on adaptation level, and that of Festinger (1957) on cognitive dissonance, are a further indication of the need for bi-polar as well as unidirectional scales. Nor will the position that ordinal scales are directionless stand up to examination, for in the ordering of qualitative data Guttman scales are bi-polar if they rank both favourable and unfavourable attitudes, and unidirectional if they rank only favourable or unfavourable attitudes, e.g. attitude to higher salaries, or to an increase in crime.

If the position which the writer advocates is accepted, a revision of Stevens's classification of scales is necessary, and if this is so, how can the importance of opposition in psychological measurement have been overlooked? Perhaps a clue lies in Stevens's remark (1951, p. 26) that the demonstration of equality in respect of some trait or other is basic to the formation of classes of objects, and that although at the formal level, the logico-mathematical definition of classes and of equality was always the subject of much writing and argument, it was not until the 1930's that it became customary to include the postulates governing the basic relation of equality in books on logic, because the relation of equality was so *obvious*. May it not be that in psychological measurement the importance of *difference within equality* has been similarly overlooked, and that some concepts which it is commonly accepted are unidirectional would be more correctly regarded as bi-polar (Thompson, 1961), forming what Karl Menger has recently called 'a full ratio class' (Churchman & Ratoosh, 1959, p. 104)?

Menger defines a full ratio class as follows:

'For many a positive ratio class C, there exists another positive ratio C^- whose elements are in a certain opposition to those of C, somewhat as liabilities are opposed to assets, or repellent forces to attractions. The opposite to objects weighing down a scale of a balance are balloons which, when attached to a scale, lift it up. Two lifts, like two weights, can be compared with one another on a balance and combined into one lift. The class of all lifts therefore is a positive ratio class.... Two opposite ratio classes C and C^- can be included in one class $C\&$...the classes of mutually equivalent objects in C and C^- fit into $C\&$ just as two opposite rays fit into a straight line.'

If psychologists agree that the notion of a full ratio class is important, Stevens's classification of scales can be modified so that interval and ratio scales are covered by a single description. Such a change could be effected by regarding a ratio scale as simply a limiting form of interval scale in which the zero coincides with the lower extremity, and in which the constant b in the formula $x' = ax+b$, given by Stevens (p. 27) is 0. Then, no matter what kind of scale investigators proposed to use, they would always be on the alert for directionality, and would always ask in a particular instance, 'Which is more appropriate, a bi-polar or a unidirectional scale?' They would not, as is often at present the case, feel under an obligation to achieve a unidirectional scale if one is possible.

It is realized that the above suggestions are controversial. Some investigators will probably refuse to accept the notion of a full ratio class, as envisaged by the writer, on the grounds that an absolute zero cannot be allocated to a purely arbitrary position. Menger himself maintains that the number 0 indicating an absolute zero is, although identical in form, not conceptually the same as the 0 which appears in the middle of a bi-polar continuum, complaining that scientists in physics seem to be less aware than psychologists of the difference between nil in mensuration and zero in graduation. There is, however, another point of view connected with quantum mechanics and the theory of relativity, and it can be argued that the position of even an absolute zero is not irrevocably fixed, and that a zero situated on a scale elsewhere than at the lowest extremity is just as absolute under certain circumstances as an absolute zero in the usual place. In atomic physics the situation regarding zero on the Absolute scale of temperature is becoming obscure. Ramsey (1956) describes an experiment in connexion with spin resonance in which, for a very special isolable subsystem, negative absolute temperatures were recorded. Under these circumstances, the coldest possible temperature on the scale was $+0$ and the hottest -0, with plus infinity and minus infinity indistinguishable as temperatures, and with all negative temperatures hotter than any positive temperature. It is true that Pippard (1957, p. 52) says of this experiment that it 'shows very clearly the essentially singular character of the absolute zero which was quite impassable by any means', but it is not clear at any rate to a psychologist where the zero was situated in this case on the scale; presumably under the exceptional circumstances ordinary measuring concepts did not apply. The fact that in physics, in the case of isolable subsystems, fundamental measuring concepts are thus having to be altered prompts the question whether in the macrophysical realm, e.g. in psychology, there is justification for assuming that because a zero is at the lower extremity of an interval scale it can be regarded as automatically less arbitrary than if its position is elsewhere, or whether a zero which is not at the lower extremity is any less 'absolute' than one that is. If this is the way in which measuring theory is moving, it provides a further reason for abolishing the distinction between interval and ratio scales, and reducing the classification of scales in psychology from four kinds to three.

III. GUTTMAN SCALING TECHNIQUES

One example of the way in which psychologists are confronted with the choice of bi-polar or unidirectional measurements occurs in the construction of Guttman scales. This example is worth considering in some detail. It involves the ways in which the responses can be arranged (either in a parallelogram or a triangle), and what is at first sight merely a difference in form can be shown to involve a difference between bi-polar and unidirectional measurement. In Guttman scaling procedures the respondents or testees are ranked in terms of total score, and the attitude or test items arranged in order of response frequency, in such a way that if an individual's total score is known, his rank order in relation to other people is also known, as well as which items he has accepted and rejected (passed or failed). The extent to which this is achieved is expressed by the coefficient of reproducibility. In constructing a Guttman scale, the responses or test answers for each individual are set out in rows and the items (i.e. questions) are arranged in columns so as to rank simultaneously persons in terms of total score and item alternatives according to frequency of endorsement. For a simple and up-to-date description of the procedure see Trenaman (1960). The data as scaled are often dichotomous, as in the course of scaling multiple alternatives are often combined, but Guttman (1950) gives the following items with several alternatives that can be regarded as typical. In each case the respondent is required to indicate the statement which he considers most applicable; he endorses one alternative only.

Question 1 (p. 125)

Which of the following would you say best applies to your job?

(i) Time always passes quickly (favourable response).
(ii) Time passes quickly most of the time (favourable).
(iii) Enjoy working part of the time, but it drags at other times (neutral).
(iv) Time drags most of the time (unfavourable).

Question 2 (p. 267)

Do you think the Army has tried its best to see that men get as square a deal as possible?

(i) Yes, it has tried its best (favourable).
(ii) It has tried some but not hard enough (neutral).
(iii) It has not tried at all (unfavourable).

In scaling, the data are represented by ball bearings or lead shot, if a scalogram board is used, although some investigators prefer to make marks with talc on Perspex frameworks (Trenaman, 1960). The data as represented are made to conform to one of two visual patterns, a parallelogram or a triangle. Although in accordance with what has already been said, it is suggested that the parallelogram is bi-polar and should be obligatory if there are both favourable and unfavourable responses, and that the triangular pattern is unidirectional and should be reserved for data in which there are overwhelmingly favourable or unfavourable responses only, most investigators, e.g. Handyside (1960) and Trenaman (1960), in so far as they notice there is a difference at all, tend to imply that both arrangements are aspects of the same thing. In spite of this the writer argues as follows. Although with dichotomous items the difference is not immediately evident, as a positive score of one type, e.g. for success, automatically excludes a score of the opposite type, i.e. for failure, the need to distinguish carefully between parallelogram and triangular arrangements becomes clearer when there are several alternatives to each question and the responses are no longer mutually determining. The use of multiple alternatives brings out a difference which is already there; the parallelogram arrangement ought not to be regarded as an arbitrary variation of triangular arrangement or vice versa; both exist in their own right and have their own implications. Other investigators, with the exception of Burt (1953) have not made sufficient distinction, Torgerson (1958) and Coombs (1953, 1960), for example, saying little on the subject, which needs the following clarification.

Table 1. *The triangular and parallelogram arrangements produced by unidirectional and bi-polar scaling*

	Test items							Totals for	
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	Shaded triangle (unidirectional)	Parallelogram (bi-polar)
Item scores	+3	+2	+1	0	−1	−2	−3		
Persons									
A	1	1	1	1	0	0	0	+6	+6
B	0	1	1	1	1	0	0	+3	+2
C	0	0	1	1	1	1	0	+1	−2
D	0	0	0	1	1	1	1	0	−6
Frequencies	1	2	3	4	3	2	1		

N.B. (*a*) In the body of the table there are two kinds of response, '1' indicating endorsement or success, and '0' no endorsement or failure.

(*b*) Looking outward from the middle of the parallelogram, the items are arranged in order of increasing 'difficulty'.

(*c*) The persons are arranged in order of their total score (attitude or ability).

(*d*) In a complicated scalogram, a population of 100, and 20 or more items with several alternatives might be used.

In Table 1, the parallelogram arrangement of responses is shown, favourable responses (acceptance or pass) being represented by 1's, and unfavourable responses by 0's. It would also be possible (not necessarily permissible, however—that would depend on the data) to use a triangular arrangement like the shaded portion of the table, but bigger, as an investigator starting *ab initio* to use the triangular arrangement with appropriate data would use *all* of the data in constructing his triangle; the shaded portion of the table is smaller than it actually would be only because the purpose of the table is to illustrate parallelogram and triangular arrangements simultaneously. Bearing in mind this limitation, certain differences are discernible. Although the items in the table are dichotomous, and each person's total score appears only once (i.e. the arrangements illustrated are the simplest possible) the order of the items in the two arrangements differs in an important respect and it must be remembered that calculation of the coefficient of reproducibility depends upon item arrangement (1's falling outside and 0's falling inside the parallelogram or triangle being counted as errors). Briefly the parallelogram arrangement conceals what amounts to a reversal of item order, whereas the triangular arrangement does not.

Thus, when Guttman uses the parallelogram arrangement, although he does not employ negative scores and merely ranks the item responses, say, 3, 2, 1, these scores are really equivalent to 3, 2, 1 in one-half of the attitude continuum and -1, -2, -3 in the other, as the alternatives are so ordered as to ensure a logical transition from positive to negative when passing from one side to the other. The order of responses is altered approximately midway along the continuum in relation to relative frequency of endorsement, being from the *least* frequent *favourable* response on the extreme left, to the *most* frequent *favourable* response near the centre, followed by the *most* frequent *unfavourable* response, and finally the *least* frequent *unfavourable* response. Ford has since referred to this arrangement as 'Guttman's staircase order' (Riley, Riley & Toby, 1954). In the triangular arrangement, however, this reversal does not occur, and Guttman's use of positive numbers only, for rank, is logically consistent.

It may be objected that in spite of what the writer says there can be no difference between the parallelogram and triangular arrangements because Burt (who was the first to suggest that the use of parallelogram and triangular arrangements of responses might be relevant to the data) factor-analysed a table of hypothetical scores arranged in typical parallelogram manner, having the 1's in adjacent columns and the 0's arranged separately, and obtained in this way factors identical with those derived from a hypothetical set of 1's arranged in hierarchical order forming a triangle (Burt, 1953).

The conclusion that parallelogram and triangular arrangements yield exactly the same coefficient of reproducibility does not, however, necessarily follow, for Burt (i) restricted himself to dichotomous items (like those in the table) and (ii) did not allow for ties, and the removal of either restriction introduces complications. In Burt's parallelogram, individual patterns of personal scores appear only once, but in Guttman's illustrations of the parallelogram arrangement (Guttman, 1950), the patterns of scoring for individuals (as shown by the rows) are repeated even for dichotomous items. Guttman advocates the replication of 'scale types' (1950, p. 75). These, according to Trenaman (1960, p. 49), may be as numerous as the degrees of freedom between the item alternatives. White and Saltz (1957) have further pointed out that in the triangular arrangement, if there are any ties in total scores or in the number of subjects passing test items, the arrangement of the matrix even with dichotomous items may not be unique, and that under such circumstances the diagonal separating the unity entries from the 0's will not be exactly coincident with the main diagonal. It is not correct, therefore, for investigators to insist that the triangular arrangement is only a simple variation of the parallelogram arising through the redundance of the unfavourable half of the parallelogram with dichotomous items, as did Loevinger (1948) in expressing a preference for Ferguson's triangular checker-board (Ferguson, 1941) and as Ford appeared to think when, in demonstrating the 'staircase order' of Guttman's items, he devised a punch-card procedure for scaling dichotomies by constructing one half of the parallelogram first and adding the second only as an afterthought (Riley *et al.* 1954, chap. 12).

Marder (1952) has constructed apparatus for scaling items with several alternatives by triangular analysis, claiming that there are advantages accruing from the triangular arrangement which are not associated with the parallelogram. He calls his procedure the 'Method of Linear Segments', pointing out that when it is used for dichotomous items the responses form a perfect triangle (the technique evidently admits no repetition of scores). Marder claims that when used with items having several alternatives, the 'Method of Linear Segments' is superior to scaling procedures involving parallelogram arrangements of responses because: (1) it gives a criterion of separation of 'light' and 'shade' which as a palpable indication of scalability is superior to that of general parallelogram shape, (ii) it simplifies the essential operations of scaling, which under Guttman's system are so complicated that operators tend to take short-cuts, and (iii) it cuts down the labour involved. Using Marder's method, Toby has evolved a punch-card technique for the triangular scaling of multiple alternatives (Riley *et al.* 1954).

It is possible that *since cumulative scaling was first introduced* insufficient distinction has been made between parallelogram and triangular arrangements of the data. Unlike the writer, Marder gives no indication that he would restrict the triangular arrangement to data which do not involve a suggestion of opposites; this confirms the likelihood of an historical omission, a supposition which receives further support from the illustrations in Guttman's earlier papers (1944, 1947*a* and *b*). Although on a casual reading the descriptions there given of the scalogram-board technique, and of the Cornell trial-scoring and graphic tehnique, give the impression that identical arrangements of items were used, there *was* a difference; with the scalogram board positive and negative responses were arranged on opposite sides of the response continuum in 'staircase order', but in the Cornell technique there was no logical shift from positive to negative half way across, the alternatives being grouped together within each item for the purpose of calculating the coefficient of reproducibility, instead of being separated into favourable and unfavourable on either side of the attitude continuum.

There may, moreover, be some connexion between the modification of the usual parallelogram procedure in the introduction of the Cornell technique, and the use of Likert items in which content and intensity are scored in the same question.

Guttman (1950) gives the following Likert-type item:

Do you agree with the following statement?

Being a W.A.C. is bad for a girl's reputation
(check one).
Strongly agree..........
Agree..........
Undecided..........
Disagree..........
Strongly disagree..........

Because they attempt to measure both content and intensity at the same time, Likert items, such as the example above, lead to confusion in respect of directionality (Thompson, 1961), and have proved difficult to scale (Guttman, 1950). Possibly the parallelogram procedure involving 'staircase order' may have been modified for such items in an attempt to eliminate logical inconsistency.

This may well be, as other difficulties also arise with Likert items if the full parallelogram procedure is applied. In an analysis of a large amount of data, the responses of individual persons (arranged in rows) occupy portions of the response continuum which, when viewed in relation to the overall parallelogram pattern, will

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be transferred successively to the right, the lower a person's total score (see Table 1). Although such an arrangement does not lead to a contradiction with most types of scoring, it does lead to a contradiction in the case of Likert items, for it implies that people who strongly agree with statements with which few other people strongly agree are neutral towards or at any rate only moderate in their disagreement with statements with which the majority of other people moderately disagree. (We are, of course, here looking at the left half and centre of the Guttman board, but the statement is also true if one substitutes 'disagree' for 'agree' and vice versa, and looks at the centre and right half of the parallelogram.) These are dubious assumptions, as experience suggests that people who are prepared to endorse statements which are so extreme as to require the expression of a high intensity of attitude are also prone to be more emphatic than other people in their rejections; there is no evidence to justify assumption of the existence of two kinds of people, (i) those who strongly affirm but only moderately disagree, and (ii) those who strongly disagree but only moderately affirm. In the Cornell technique, as used originally, the difficulty seems to have been avoided by adopting a somewhat different item arrangement, and it may well be that the use of Likert items, with the introduction of the Cornell technique, led to modifications of procedure which have since helped to obscure the full implications of parallelogram and triangular arrangements of the data.

IV. BI-POLAR AND UNIDIRECTIONAL MEASUREMENT IN PSYCHOLOGY AND PHYSICS

If they are able to choose one or the other, most British psychologists tend to prefer unidirectional to bi-polar measurement, probably as a result of the prestige of such measures as the standard metre in physics. Apart from the position held by the linguistic psychologists the situation is similar in the United States; in factor analysis, for example, Thurstone expressed a preference for a positive manifold (without, in the writer's opinion, giving a fully convincing explanation). There is, however, another side to the picture; some American psychologists with a psychoanalytic leaning, e.g. Ruesch and Bateson (1951), regard almost all psychological activities and processes as having an essentially bi-polar character. Such psychologists attach importance to perceptual gestalten and to the dynamical aspects of social psychology, regarding information, perception and values as virtually inseparable (and not without a certain amount of support for their position; see chaps. 7 and 9). Bi-polar and unidirectional measurement are both needed in psychology, and it is an unnecessary constraint always to seek the latter, since whether bi-polar or unidirectional measurement is more appropriate depends not only on the nature of the data but also on the investigators' theoretical approach.

An interesting comparison can be drawn between physics and psychology regarding the necessity for double-category as well as single-category measures and for bi-polar and unidirectional measurement. On some occasions the distinction is admittedly one of convenience, but on others there is no doubt what the choice has to be. Thus it is permissible, as a matter or convenience both in physics and psychology, when calculating product-moment correlation coefficients, to convert unidirectional to bi-polar measurements by taking deviations from the mean, and then to restore the unidirectional form in the course of squaring the deviations. Permissible changes, such as these, made during the course of computation, are, however, not comparable with the constraints on the measuring system which are sometimes necessitated by the nature of the data, either as theoretically postulated or empirically determined. An example in physics has been the recent attempt to verify 'anti-gravity' (gravitation in this context being regarded as repellent as well as attractive). In physics the importance of assumptions concerning polarity are also illustrated by Dirac's assumptions in wave mechanics, which have led to the experimental discovery of positive electrons and other particles, it now being one of the guiding principles of nuclear physics that wherever a positively or negatively charged particle exists, there must also exist a particle of opposite charge (Born, 1935, p. 44). In psychology, assumptions regarding polarity are similarly of heuristic value.

Conclusions

It is not realistic to expect single homogeneous categories to suffice for every investigation, nor to try to express all measures by unidirectional scales. Both in psychology and physics, relationships that entail dynamic opposition and which require expression by means of subcategories or subdivisions within scales are commonplace. Although when and where each is appropriate depends in part on the theoretical framework of the investigator, unidirectionality and bi-polarity are not entirely subjective. Sometimes the contrast or opposition inherent in the data is so pronounced that measurement *has* to be bi-polar, and unless both kinds of measurement are allowed for important discoveries may be indefinitely delayed. In view of the tendency in psychology to use unidirectional measurement too frequently, and of developments in physics where a general loosening of the metric is apparent (Burt, 1958), the distinction between interval and ratio scales is sometimes unnecessary and scales can under such circumstances be described as bi-polar or unidirectional, and cross-classified as nominal, ordinal and interval.

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SIZE CONSTANCY AND CUE RESPONSIVENESS IN PSYCHOSIS

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An experiment is reported designed to re-assess size constancy behaviour and some of its co-relatives in psychotic and non-psychotic subjects. The predictive basis of reduced size constancy for schizophrenic and non-schizophrenic psychotic patients was found in the clinical evidence characterizing these conditions and in Piaget's genetic theories of cognitive development. It was postulated that there would be no significant difference in size constancy between different psychotic groups or between different non-psychotic groups, but that the psychotic groups would show significantly reduced size constancy compared with the non-psychotic sample. It was further postulated that low size constancy would be positively related to reduced cue responsiveness operationally defined in several different ways. Size constancy and cue responsiveness in relation to familiar and non-familiar objects are assessed. The results are taken to provide a considerable degree of support for the hypotheses, and are explained in terms of peripheral as well as central mechanisms. The findings do not support previous evidence suggesting a causal relationship between low size constancy and under-estimation of distance.

INTRODUCTION

Our ability to judge the size of objects is generally well developed, so that even if they are perceived at a considerable distance or in a different setting in the total stimulus field, a fair degree of accuracy will be maintained despite discrepant retinal cues. Various factors have been held responsible for maintaining constancy in the perception of size: knowledge of the class of object, distance cues which may be provided along the line of vision by other, intermediate objects; knowledge of the laws of perspective and of the visual angle (Gibson, 1950). As far as laboratory experiments are concerned, it is known that differential interpretation of instructions can lead to divergent matching in size, while horizontal angular separation of two objects to be matched for size also affects constancy (Joynson, 1958*a*, *b*).

This paper describes a further investigation into the perception of the size of objects by psychotic and other subjects. Experiments already published have shown a relationship between low size constancy and reduced reality contact (Lovinger, 1956), low size constancy in acute schizophrenics and literalness and form-boundedness in Rorschach responses (Crookes, 1957), while another investigator (Raush, 1952) found that paranoid schizophrenics tend to show over- rather than under-constancy. The relationship between personality constancy and perceptual constancy has been discussed by Scott (1959), who also found reduced colour constancy in schizophrenics. Weckowicz (1957) first obtained further evidence of reduced size constancy in chronic schizophrenia, and subsequently found that reduced size constancy correlated with systematic under-estimation of distance (Weckowicz, Sommer & Hall, 1958) and with impaired conceptual thought (Weckowicz & Blewett, 1959). Evidence that schizophrenics have higher constancy indices (in terms of closeness to veridical size) than normal people has been claimed by Raush (1952) and Perey (1961). The latter deduced his experimental hypotheses from the postulate that schizophrenics, by virtue of a cognitive process of over-inclusion, were more responsive to distance and peripheral cues in a size-matching situation. His actual findings for objective (real) size judgements show in addition, however, as do results of other workers, that schizophrenics have a greater tendency towards under-constancy than normals, who more generally tend towards over-constancy.

The present investigation had three main aims: to attempt to repeat the previous findings of under-constancy in schizophrenia and to extend them to psychosis generally; to attempt to account for the phenomenon by a general principle of cue responsiveness; and to attempt to set both the general principle and the evidence from published findings of the phenomenon within Piaget's theories of cognitive development and the psychopathology and symptomatology of psychotic illness.

Size constancy depends on the ability to integrate multiple cues, upon contact with an objective environment, and upon the ability to relate appropriate aspects of the environment to each other in a rational manner, rather than upon a segmental or atomistic approach. It also requires inferential operations on the basis of maximal objective and external cues. This statement rests on a good many studies in which size constancy has been shown to be impaired under cueless or cue-reduced conditions. In schizophrenia, contact with objective reality is tenuous or absent, logical thinking is often grossly impaired and indicative of a failure of integrative cognitive processes, and awareness of reality is made difficult because of pre-occupation with internal and subjective ideation and experience. These processes are demonstrated clinically in delusions, hallucinations and in what is often (and imprecisely) termed 'thought disorder'. Delusional and impaired logical thinking are common to all psychoses (though most marked in schizophrenia), demonstrating a general inability to use and integrate objective reality in the environment. In schizophrenia particularly, there is evidence that perception is fluid and that persons and objects appear differently on different occasions, or from moment to moment, even in the same environment—so that schizophrenic perception would appear to contain elements of inconstancy (Freeman, McGhie & Cameron, 1957; Chapman, Freeman & McGhie, 1959). But it is clear that other forms of psychosis also display a rupture of optical reality contact and show evidence of false and inadequate perception of the objective world. The impairment of perception and thinking in psychotic states thus makes the presence of optimum conditions for normal size constancy behaviour and responsiveness to perceptual cues unlikely.

In Piaget's terms reduced size constancy may be described as a failure of the process of cognitive adaptation. Piaget says that the bases for size, shape and colour constancy are laid down in early childhood during the sensory-motor phases of intellectual development (Piaget 1950, 1951, 1953, 1955), and there is experimental evidence that young children have reduced size constancy, as compared with older children and adults (Zeigler & Leibowitz, 1957). Phenomenologically, under-constancy in size judgements may be due to excessive primacy being given to isolated sensory cues; genotypically under-constancy suggests the immaturity or dysfunction of the appropriate perceptual schemata and the cognitive operations which the schemata represent as well as subserve.

Attention has already been drawn by Freeman *et al.* (1957) and Freeman & McGhie (1957) to the similarities between the perceptual and conceptual phenomena of schizophrenia and the perceptual and thinking processes of infants and young children described by Piaget in his hierarchical system. This analogy must not be taken too far, but there would appear to be sufficient systematic observational evidence for the contention that psychotic patients are frequently unable to proceed beyond the primary sensory meaning of stimuli to valid inferential behaviour which must be based on response to all relevant and interrelated available cues. Quite apart from the symbolic and subjective meaning of psychotic behaviour, the evidence would seem to indicate that it shows a condition of social, motivational and cognitive primitivation, fragmentation and dissociation, in which the individual can respond to his environment only with behaviour appropriate to the level of functional competence available to him in his illness. If this level is low, as by definition it is, then responses are likely to be maladaptive, and not even a considerable increase in the amount of objectively available environmental information or cues will lead to marked modifications of behaviour.

On the basis of these several considerations it was predicted that psychotic subjects would show under-constancy in their judgement of the size of objects at a distance as schizophrenics did in the studies cited above, on different theoretical grounds; that psychotics would have greater difficulty in modifying their judgements as between cueless and full cue experimental conditions; and that there would be no significant differences in size constancy or cue responsiveness between different psychotic subgroups.

DESIGN AND METHOD

Hypotheses

The following hypotheses were tested:

(1) Chronic non-paranoid schizophrenics have reduced size constancy for a variety of objects compared with normal and non-psychotic subjects.

(2) Size constancy of chronic non-paranoid schizophrenics does not differ significantly from the size constancy of paranoid schizophrenics and of other psychotics.

(3) Normal subjects and non-psychotic patients do not differ significantly in size constancy.

(4) Size constancy of a combined psychotic sample of patients is significantly reduced compared with the size constancy of a combined normal and non-psychotic group of subjects.

(5) Non-paranoid schizophrenics and psychotics generally show reduced responsiveness to perceptual cues in a situation requiring constancy in the perception of size.

Apparatus and procedure

Subjects were taken into a darkened room curtained off at the subjects' end so that its length could not be estimated. They were tested at a table painted a matt black 3·10 m. long, 70 cm. wide, and 79 cm. high, on which a three-sided framework of the same size as the table had been erected to produce a 'tunnel'. The black cloth covering of the frame prevented all light from entering its interior. At the subjects' end of the box was a flap which could be operated by the investigator by remote control. When the flap was opened, subjects with their chin in a chin rest were confronted by the stimulus object (standard) at a distance of 1 m. and the variable at a distance of 3 m. Stimulus and variable were individually and dimly illuminated at an intensity allowing the objects to be clearly seen but without giving any other cues. The objects (standards) requiring size judgement were two diamond shapes with diagonals of 10 and 15 cm., a rod

15 cm. high, 1·2 cm. diameter, all painted white, a colourless medicine bottle (13·4 cm.), a shiny penny (3·1 cm.), a box of 20 cigarettes ('Players Weights') placed diagonally (10·4 cm.) and the Ace of Hearts, normal playing card size (8·9 cm.). The seven objects were presented in random order. 'Familiar and homogeneous' as well as 'non-familiar and heterogeneous' objects were chosen. This was done because McKennell (1960) had shown that non-visual factors such as prior knowledge and memory may entirely determine the size estimate of familiar objects.

At the end of the series of judgements in the 'tunnel', the curtains were opened, the framework dismantled, and the series was repeated under normal daylight and full cue conditions. The first set of conditions will be referred to as 'cueless', and the second as 'cues'. Each object was judged four times, with the starting point of the variable alternately at the maximum or minimum point of its range. Horizontal and vertical angular separation between stimulus and object was 5° and the instructions insisted on binocular fixation. For the diamonds, size judgements were made with a diamond variable in size by the experimenter according to instructions from subjects; for the other five objects the variable was a white rod at the 3 m. mark which could be lengthened or shortened by the subject by means of a handle conveniently placed at his end of the table.

Following Joynson's (1958*a*) example, the instructions did not draw attention to the possibility of two different kinds of judgements: behavioural or phenomenal. Although Joynson had found that behavioural or objective size judgements predominated even with ambiguous instructions, it was of some interest to verify his findings with different samples. Subjects were therefore merely asked to set the variable to the same size as the standard object. The actual method adopted was elicited by prolonged questioning at the end of the experiment.

Subjects

The study was carried out on 20 chronic non-paranoid schizophrenics, mean age 39·0 years, mean chronicity 7·9 years and mean I.Q. 106·2; 10 paranoid schizophrenics, mean age 43·9 years, mean chronicity 5·9 years, and mean I.Q. 95·9; 4 manic-depressive psychotics, mean age 51·3 years, mean chronicity 0·6 years and mean I.Q. 97·5; 10 mixed short stay neurotics, mean age 43·4 years and mean I.Q. 109·1; and 12 normal controls, mean age 34·5 years and mean I.Q. 117·5. Recent diagnoses by a consultant psychiatrist were accepted for the patient groups. Only co-operative patients able to carry out the instructions and able to report on their experimental actions were included, and the sample was restricted to men. Some of the patients had taken part in other psychological investigations, others were tested subsequently to clinical psychological assessment. The normal control group consisted of professional, clerical and nursing personnel in two hospitals. In all cases where no precise measure of intellectual capacity was available, intelligence was estimated by Vernon's norms of Progressive Matrices scores for different occupational groups (Vernon, 1949).

A major drawback of the study is the small number of non-schizophrenic psychotics of appropriate age. The determining factor here was the generally low admission rate for affective psychoses within the age range of the present study and their relatively quick remission. Any evidence for hypothesis 2 is therefore bound to be tenuous. It was decided, however, that if the difference between the non-paranoid schizophrenics and the other psychotic groups was not significant, (hypothesis 2), these groups could be legitimately combined into a single and larger psychotic group to test hypothesis 4.

The differences in intelligence are significant at beyond the 5 % level when comparing normal controls with non-paranoid and paranoid schizophrenics, and when comparing the combined normal control and neurotic groups with the total psychotic group. While it is preferable for most investigations to have groups equated for intelligence, such a procedure is obviously difficult when one group consists of deteriorated schizophrenics of considerable chronicity. In any case, even the pairing of schizophrenic and normal or neurotic subjects for I.Q. on any standardized test does not ensure that their intellectual capacity will operate comparably in all cognitive or perceptual situations. As far as the present investigation is concerned, it will be seen that the obtained differences in intelligence in no way invalidate the findings of the study.

RESULTS

The index of size constancy used in this study is a 'constancy ratio' obtained by dividing individual judgement value by the size of the object being judged, and the terms *Constancy Ratio Cueless* and *Constancy Ratio Cues* refer to size constancy indices obtained under cueless and full cue conditions respectively. Traditionally, the more complex size constancy indices developed by Thouless and Brunswik have been used, but it seemed more economical to the present writer to use the simpler index without thereby detracting from the experimental data. In the ratios used here, high values indicate reduced constancy and low values over-constancy, while an index of 1·00 describes full constancy. Considerations of space make it impossible to present all the data in Table form, but relevant data thus omitted are discussed in the text.

Interpretation of instructions

The decision as to how the test instructions were to be interpreted was left to the subjects (see above). Of the 56 subjects 13 (9 psychotics and 4 normal controls) said that they had made their judgement on the basis of apparent or image size. The remainder of the sample said that they had made their judgement taking distance into account and a few were unable to describe sufficiently precisely how they arrived at a size judgement of subjective equality. Several schizophrenic subjects additionally reported that standard and variable appeared unequal to sight but that they expected them to be equal on measurement. It is thus possible that the kind of analytic approach to size judgement and an awareness of its distinctiveness compared with 'actual size' judgements, as described by Joynson, is more difficult for patients. Perey (1961) has made a similar observation.

Comparing the constancy ratios obtained under cue conditions from the non-paranoid schizophrenics who carried out an 'apparent size' match or judgement, with the constancy ratio of those who made an 'actual size' judgement, no significant differences relatable to interpretation of instructions were obtained. In fact differences beween objects were greater than differences between methods of judgement. For the normal control group no significant co-variance was found between 'apparent size' judgements and under-constancy.

Size constancy

The constancy ratios obtained under cueless and cue conditions are summarized in Table 1.

The non-parametric Mann–Whitney U-test was used to calculate the significance of the differences since an examination of a sample of scattergrams revealed distributions not conforming to normality. The differences between normal controls and neurotics, and the differences between non-paranoid schizophrenics, paranoid schizophrenics and manic-depressive psychotics were not significant. These results verified hypotheses 2 and 3, and provided the justification for combining the psychotic and non-psychotic subgroups into two larger groups for the purpose of testing hypotheses 3 and 5. Between-group tests of significance of differences are summarized in Table 2. Table 2(*a*) shows the significant differences between the groups for size constancy under full cue conditions. Table 2(*b*) shows the results of the between-group tests of significance for cueless conditions. (Results for five of the seven objects are here omitted because none of them was significant.) The present study verified the findings of other investigators that in the absence of distance and other cues the size judgements even of normal control subjects are liable to considerable error. The greater reliance on retinal cues in these conditions is reflected in predominantly high over-constancy values.

An additional description of the differences between the psychotic and non-psychotic groups is obtained from Table 3, in which the evidence for over- as well as under-constancy is summarized. For 2×2 contingency tables with D.F. = 1, comparing those giving judgements of -10% or more of object size with those giving judgements of $+10\%$ or less, Chi square values show that the size judgements of the combined psychotic and the schizophrenic samples are not only marked by the presence of under-constancy but also by the absence of over-constancy.

Consistency of size judgements

On the basis of previous work (Hamilton, 1957) it was expected that subjects would show some degree of personal consistency in their judgement of the size of different objects. Rank-difference correlations were calculated for the non-paranoid schizophrenic and the combined normal and neurotic samples. Of these 19 out of 21 and 9 out of 21 respectively were significant for the two groups. Correlations between Constancy Ratio Cues and Cueless were also computed. These are shown in Table 4.

Table 1. *Means of constancy ratio cueless and cues*

	All psychotics	Neurotics + normal controls	Non-paranoid schizo-phrenics	Normal controls	Neurotics	Paranoid schizo-phrenics	Manic-depressive psychotics
Diamond I							
Cueless	1·09	1·04	1·07	1·08	0·99	1·13	1·11
Cues	1·13	1·01	1·13	1·01	1·01	1·17	1·02
Diamond II							
Cueless	1·20	1·13	1·16	1·21	1·04	1·26	1·24
Cues	1·24	1·09	1·24	1·15	1·04	1·29	1·13
Cigarettes							
Cueless	1·11	0·99	1·18	1·12	0·86	1·08	0·80
Cues	0·97	0·86	1·01	0·88	0·83	0·94	0·88
Rod							
Cueless	1·06	0·92	1·04	0·98	0·85	1·13	0·95
Cues	1·03	0·87	1·04	0·90	0·82	1·06	0·88
Playing card							
Cueless	1·30	1·14	1·36	1·14	1·15	1·29	1·05
Cues	1·17	1·02	1·19	1·28	0·97	1·19	0·98
Penny							
Cueless	1·47	1·29	1·56	1·05	1·56	1·45	1·11
Cues	1·07	0·90	1·08	0·91	0·90	1·14	0·83
Bottle							
Cueless	1·02	0·96	1·03	0·93	0·98	1·04	0·89
Cues	1·02	0·87	1·03	0·88	0·86	1·02	0·92

Table 2. *Between-group tests of significance*

	Ps *v.* NoNe	Sc *v.* NoNe	Ps *v.* No	Ps *v.* Ne	Sc *v.* No	Sc *v.* No	Sc *v.* Pa	NoNe *v.* Pa†	No *v.* Pa†	Ne *v.* Pa†
				(*a*) Constancy ratio cues						
Diamond I	197*	117½*	93½*	103½	64½	53	—	43*	21*	22
Diamond II	172*	110*	122½	49½*	73	37*	—	36½*	26½	10*
Cigarettes	263	145½	—	95½	—	52	—	—	—	24½
Rod	211*	113½*	133½	77½*	68½	44½*	—	50½	—	15*
Playing Card	—	—	—	—	—	60½	—	—	—	—
Penny	222*	117½*	120	96½	60*	53½	—	48½	27	21½
Bottle	276	—	—	—	—	—	—	—	—	25½
				(*b*) Constancy ratio cueless						
Cigarettes	—	150	—	109½	—	57	—	—	—	—
Penny	274	146½	123½	—	64	—	—	—	—	—

Ps, All psychotics; No, normals; Ne, neurotics; Sc, non-paranoid schizophrenics; Pa, paranoid schizophrenics.

* Denotes a Mann–Whitney *U*-test value significant at 1 % level or better, plain figures denote significance at 5 % level or better. Non-significant data are omitted.

† Two-tail tests of significance. All other tests are one-tail.

Table 3. *Group differences of over- and under-constancy*

Cut-off points: judgements of ± 10 % of object size under cue conditions

Groups compared:	Diamond I	Diamond II	Cigarettes	Rod	Playing Card	Penny	Bottle
Ps *v.* NoNe							
χ^2	10·64	—	4·33	6·34	0·74	5·47	3·23
P	$< 0{\cdot}01 > 0{\cdot}001$	0·79*	$< 0{\cdot}05$	$< 0{\cdot}02$	N.S.	$< 0{\cdot}02$	N.S.
Sc *v.* NoNe							
χ^2	4·12	—	4·82	6·93	0·78	7·04	2·07
P	$< 0{\cdot}05$	0·36*	$< 0{\cdot}05$	$< 0{\cdot}01$	N.S.	$< 0{\cdot}01$	N.S.

* Fisher's exact probability test, because of small number of over-constancy scores.

Cue responsiveness

Hypothesis 5 requires the study to show that non-paranoid schizophrenics and psychotics generally show less cue responsiveness than the non-psychotic groups. Cue responsiveness is here operationally defined as modification of response when changing over from cueless to cue conditions of judgement. Evidence of cue responsiveness thus defined may be obtained in a number of ways. A so-called Responsiveness Index may be calculated by dividing change in judgement with cue conditions irrespective of sign, by the amount of deviation of judgement from accuracy under cueless conditions irrespective of sign. This index is, of course, no more than one possible way of relating amount of change in judgement to previous deviation from accuracy of judgement. Differences in cue responsiveness may be further demonstrated by comparing for significance of differences correlations between Constancy Ratio Cueless and Cues for the separate experimental groups, using Fisher's z-transformation method, as well as by assessing the significance levels of the individual correlations.

Table 4. *Correlations between Constancy Ratio Cues and Cueless*

	Diamond I	Diamond II	Cigarettes	Rod	Playing Card	Penny	Bottle
NoNe	+0·388	+0·563**	+0·387	+0·168	+0·146	+0·048	+0·154
So	+0·690**	+0·702**	+0·267	+0·643**	+0·395	+0·776**	+0·445*
Between-correlation significance $P =$	N.S.	N.S.	N.S.	> 0·05 < 0·10	N.S.	< 0·01	N.S.

Significance of individual correlations: $*P \leqslant 0{\cdot}05$, $**P \leqslant 0{\cdot}01$.

Table 5. *Responsiveness Index means*

	All psychotics	Neurotics + normal controls	Non-paranoid schizo-phrenics	Normal controls	Neurotics	Paranoid schizo-phrenics	Manic-depressive psychotics
Diamond I	3·41	1·36	1·51	1·59	1·07	8·11	1·12
Diamond II	1·84	3·03	1·87	4·94	0·75	1·51	2·51
Cigarettes	2·09	0·69	1·66	0·74	0·64	3·67	0·27
Rod	1·17	1·96	1·05	2·77	0·98	1·67	0·55
Playing Card	0·96	1·02	1·10	0·97	1·07	0·83	0·59
Penny	1·24	1·14	0·87	1·22	1·05	1·26	3·02
Bottle	0·97	2·39	1·17	3·30	1·30	0·65	0·79

Table 6. *Group difference in Responsiveness Index*

	Ps v. NoNe	So v. NoNe	Ps v. No	Ps v. Ne	So v. No	So v. Ne	NoNe v. Pa†	Ne v. Ps†
Diamond I	—	152	—	—	—	—	—	—
Cigarettes	263	137½	—	—	—	55½	—	28
Rod	276	152½	138	—	77½	—	—	—
Bottle	235	—	—	95	—	—	51	17

Figures denote a Mann–Whitney U-test value significant at 5 % level of better. No-significant data are omitted.

† Two-tail tests of significance. All other tests are one-tail.

Cue responsiveness may be expected not only in relation to size judgements but also in distance judgements. Reduced cue responsiveness postulated for psychotics would therefore be expected also in terms of greater invariance of judgements of distance comparing cueless and full cue conditions of presentation. It could, furthermore, be argued that cue responsiveness is operating when objects of different kinds and sizes are judged, so that the greater consistency of size judgements obtaining in the non-paranoid schizophrenic group will have to be reconsidered below on the basis of that assumption.

Table 5 shows the Responsiveness Index values, while the significant differences are tabulated in Table 6. With the index used here, a higher figure indicates a greater degree of modification of size judgement when operating under cue conditions and a low Responsiveness Index is, therefore, taken to indicate a relative absence of cue responsiveness. A greater Responsiveness Index could also be obtained by having initially a smaller degree of error in size constancy under cueless conditions of judgement with a proportionately small adjustment under cue conditions. The assessment of cue responsiveness by this method shows on the whole greater responsiveness by the non-psychotic groups, and in addition somewhat greater responsiveness by the paranoid schizophrenics than the non-paranoid schizophrenics. For the Packet of Cigarettes significantly greater cue responsiveness was shown by the psychotic subgroups.

Table 4 above shows the correlations between the Constancy Ratio Cueless and Cues for the combined non-psychotic sample and the non-paranoid schizophrenic group. In six of seven cases the correlation is higher for the schizophrenic group. In addition, 5 of the 7 correlations between Constancy Ratio Cueless and Cues are significant for the non-paranoid schizophrenic group, whereas only one of these correlations is significant for the non-psychotic group.

Table 7. *Mean distance judgements of* 3 *m. and group differences*

Judgements are in feet—3 m. = 9·84 ft.

	Ps	NoNe	Sc	No	Ne	Pa	M-d Ps
Cueless	11·86	11·67	9·60	11·46	13·08	17·80	8·38
Cues	9·81	10·34	8·33	10·16	10·56	12·20	10·00

	Ps *v.* NoNe	Sc *v.* NoNe	Sc *v.* No	Sc *v.* Ne	No *v.* Ne
Cueless					
U	231	117	74	40½	53½
P	< 0·01	< 0·01	< 0·05	< 0·01	N.S.
Cues					
U	263½	112	64	43	57
P	< 0·05	< 0·01	< 0·05	< 0·01	N.S.

Distance judgements

At the end of the cueless as well as of the cue conditions of presentation, subjects were asked to make a verbal statement about the distance of the variable position from the edge of the table nearest to them. The means of distance judgements and group differences are given in Table 7. The non-paranoid chronic schizophrenics show some under-estimation of the distance, especially under cue conditions, but the group of paranoid schizophrenic over-estimated considerably. The significance levels are calculated with one-tail tests of significance between the psychotic and non-psychotic groups in view of the available evidence of under-estimation of distance by schizophrenics (Weckowicz, 1957).

There are other data on the relationship between size and distance judgements relevant to the present investigation. For instance, the correlation between distance judgement under cueless and under cue conditions was $+0{\cdot}937$ for the non-paranoid schizophrenic group and $+0{\cdot}551$ for the combined non-psychotic group, the between-correlation difference being significant beyond the 1% level. There was only one significant correlation between the constancy ratios and distance judgements under cueless and cue conditions respectively for the combined non-psychotic sample and the non-paranoid schizophrenic group, and 79% of the correlations were negative.

Considering cue responsiveness as shown in changes in distance judgements under the two experimental conditions, modifications from cueless to cue conditions of judgement in the direction of greater accuracy were more frequently carried out by the non-psychotic subjects. The difference between non-paranoid schizophrenics and the combined non-psychotic group was significant beyond the 5% level ($U = 153$, one-tail), whereas the difference between the total psychotic group and the non-psychotic group was in the direction of greater responsiveness for the latter group ($0{\cdot}10 > P > 0{\cdot}05$, one-tail). The difference between the two non-psychotic subgroups was not significant. All correlations between modification of distance judgement and the size constancy ratios under cue conditions were negative and fell short of significance.

Familiar and non-familiar objects

Following the findings of McKennell (1960), the battery of objects for the present size constancy study was assembled with a view to allowing some assessment of the effect of memory of familiar and relatively homogeneously sized objects in the postulated low constancy behaviour of the psychotic sample. For the purpose of this comparison constancy ratios and Responsiveness Indices were averaged for the three non-familiar and non-homogeneous objects: the two Diamonds and the Rod, and for three of the four familiar and homogeneous sized objects: Packet of Cigarettes, Playing Card and Penny. The Medicine Bottle was excluded because it must be considered a familiar but non-homogeneously sized object.

It was predicted that the postulated perceptual under-constancy of psychotics, compared with the non-psychotic group, would be present in the size judgements of both familiar and non-familiar objects, that it would be greater for unfamiliar objects and that the difference in constancy ratios under cue conditions between the psychotic and non-psychotic samples would be greater with objects of which no definite size memory was available. It was also predicted that the non-psychotic group would show less under-constancy for the familiar objects. The first, third and fourth predictions were confirmed. The averaged Constancy Ratio Cues of the total psychotic group and the non-paranoid schizophrenic subgroup were significantly greater than those of the non-psychotic group ($P < 0{\cdot}05$ for familiar objects and $P < 0{\cdot}01$ for unfamiliar objects), and the Constancy Ratio Cues of the non-psychotic group was significantly greater for non-familiar objects ($P < 0{\cdot}05$). While the total psychotic group and non-paranoid schizophrenic sub-group showed greater under-constancy for non-familiar objects, the difference between familiar and non-familiar objects fell short of significance. A prediction of greater cue responsiveness by the non-psychotic sample, particularly for familiar objects, was not statistically confirmed, though the previously indicated trends of lack of cue responsiveness by the psychotic samples were replicated when considering it on the basis of the familiar–non-familiar dichotomy.

In view of the obtained greater cue responsiveness of the psychotic group for the Packet of Cigarettes, the influence of smoking habits had to be taken into account. Comparing smokers and non-smokers of cigarettes for the separate non-paranoid schizophrenic and non-psychotic groups, no significant differences were found either for constancy or for cue responsiveness for the schizophrenic group, but significantly greater *over-constancy* was found for smokers of the non-psychotic group ($P < 0{\cdot}05$).

No significant relationships for the main experimental groups between cue responsiveness as assessed by the Responsiveness Index and size constancy under cue conditions were found. Further calculations were therefore carried out using the dichotomy of familiar and non-familiar objects but considering only the extreme end quartiles of the distribution of data. No significant findings were obtained for the familiar objects, but when considering the non-familiar objects, some results consistent with hypothesis were obtained. For the total psychotic group reduced size constancy was significantly associated with low cue responsiveness ($P = 0{\cdot}003$), and considering the lowest quartile of the total psychotic and non-psychotic sample combined, a positive but non-significant relationship between size constancy and cue responsiveness was also found to hold ($0{\cdot}10 > P > 0{\cdot}05$). These relationships obtained whether the extreme distributions of the size constancy ratio or of the Responsiveness Index were considered.

DISCUSSION OF RESULTS

The evidence would appear to be sufficiently conclusive for the statement that Hypotheses 1, 2, 3, and 4 have been upheld. Previous evidence that chronic and acute non-paranoid schizophrenic men show an impairment in the fundamental perceptual process of assessment of size over distance (viz. Lovinger, 1956; Crookes, 1957; Weckowicz, 1957) has thus been supported. The present small group of non-schizophrenic psychotics showed no significant differences in size constancy when compared with the other psychotic groups, but in view of the small sample of manic-depressive patients, this particular result will need to be confirmed. In view of the low chronicity range of this group, association between chronicity and impaired size constancy is contra-indicated. This suggestion is confirmed when comparing for the schizophrenic sample those of 5 years' chronicity and less with those of 6 years' chronicity and more, a division which cuts the sample almost at the median. The difference between these chronicity subgroups in size constancy or cue responsiveness for familiar as well as non-familiar objects is not significant.

Alternative hypotheses to account for the present findings concerning size constancy in terms of age and intelligence would also have to be rejected. With regard to age, none of the differences between the combined psychotic group, the non-paranoid schizophrenic group and the combined non-psychotic group is significant.

With regard to intelligence, there is no evidence that reduced size constancy is dependent on or even related to low intelligence; in fact, an inverse relationship was found first by Thouless and later by Crookes. The present sample, too, showed a fairly consistent relationship between higher intelligence and low constancy. For example, when comparing schizophrenics with an I.Q. of 110 and more, with schizophrenics with an I.Q. of 109 or less, the more intelligent sample showed greater underconstancy under cue conditions for each of the seven objects used, four of the differences being significant at the 5 % level or beyond, using a one-tail test. No significant differences were found when comparing the same two schizophrenic I.Q. subgroups for differences in distance judgement under full cue or cueless experimental conditions. Since the non-psychotic group was significantly more intelligent than the psychotic group of subjects, and since the latter showed significantly lower constancy than the former, an explanation of the present findings in terms of intelligence is untenable. There is no doubt, however, that carefully designed studies are badly needed to explain the negative correlation obtained in a number of studies between intelligence and size constancy.

The findings of generally reduced size constancy in psychotics may therefore be accepted with reasonable confidence as being representative of their perceptual behaviour in this particular area. The main task of this discussion is therefore to try to account for this behaviour in the most consistent and economical terms. This was done by Weckowicz (1958) by postulating on the basis of separately obtained distance and size judgements, that schizophrenics under-estimate distance, that they live in a 'flatter' world and that under-estimation of distance and reduced size constancy are both the result of faulty integration of the third visual dimension. It has been pointed out by Rump (1961*a*) that Weckowicz's interpretation of his findings assumed the 'Invariance Hypothesis', i.e. a reciprocity between perceived size and perceived distance. The present writer agrees with the conclusions of Rump that the 'Invariance Hypothesis' is of doubtful validity (cf. also Epstein, 1961; Epstein, Park & Casey, 1961), and was in any case not appropriately tested in Weckowicz's studies. Rump has presented elsewhere (1961*b*) data on the basis of which he cast doubt on a linear relationship between size and distance judgements. While the present study confirmed Weckowicz's findings with respect to under-estimation of distance (see Table 7), Rump's criticism of the 'Invariance Hypothesis' was supported by the almost wholly negative and wholly non-significant correlations between distance judgement and constancy ratios for the non-paranoid schizophrenic sample, and only four positive correlations (one of them significant) for the non-psychotic sample. This non-linear relationship was found to hold for both cueless and full cue conditions.

On purely rational grounds it might be suggested that differences in distance judgements provide an inadequate explanation for the finding of reduced size constancy behaviour of schizophrenics and other psychotics, mainly because distance judgement itself is a complex operation in which relational and inferential cognitive activity must be carried out on a basis of maximal cue input. The hypothetical framework of the present study specifically postulated impairment of these operations in psychotic patients, and put forward a more specific impairment of cue responsiveness to account for impaired size constancy (as well as impaired distance judgement). Cue responsivemess was operationally defined above and assessed (*a*) by means of a

Responsiveness Index, (*b*) by the differences between the correlations between Constancy Ratio Cues and Constancy Ratio Cueless, (*c*) by invariance of distance judgements when experimental conditions changed, and (*d*) by degree of consistency of size judgements for different objects. The degree of invariance of judgement by the psychotic groups following the institution of full cue conditions demonstrated on all these criteria is interpreted here as lack of cue responsiveness as defined, and as providing considerable evidence for the hypothesis. The evidence suggests then that psychotic patients, and non-paranoid schizophrenics particularly, find it difficult to improve a specific perceptual skill when average conditions for its operation become available. To some extent at least, the psychotic patients in this study appeared to operate perceptually under normal conditions as if still restricted by the earlier cueless conditions. They appeared to be less well able than the non-psychotics to modify their judgement of objects when normal field cues had been added to the isolated object stimuli. It is this incapacity, which for present purposes had been deduced from the clinical and psychological picture presented by psychotic patients, which was termed lack of cue responsiveness. The findings also suggest a considerable lack of perceptual and thus response differentiation in psychotic patients, a finding which in many ways may be similar to the greater degree of stimulus generalization of schizophrenics, for which evidence has already been presented in a different context by Rodnick & Garmezy (1957). It is possible that different results obtained by different investigators (cf. Raush, 1952; Perey, 1961) as well as the kind of intra-group response scatter obtained with the present samples of psychotic patients may be due to differences in chronicity, overall degree of illness, degree of primary cognitive disturbance or degree of institutionalization. Some or all of these factors may have accounted for the similarities between the paranoid and non-paranoid schizophrenic groups in this study. Studies attempting to control some of these variables are therefore under way in this department.

The constancy ratios and distance judgements given in Tables 1 and 7 respectively show that on occasions the mean values obtained from the psychotic groups were closer to objective accuracy than the judgements of the non-psychotic groups. The explanation would seem to lie in the consistently larger standard deviations of the psychotic groups probably produced by the uncontrolled variables mentioned above. (Closer to accuracy judgements by schizophrenics were also obtained by Perey (1961), with normal subjects showing greater over-constancy and schizophrenics showing under-constancy. Perey, however, adopts a different theoretical basis for and interpretation of his results.) The question of accuracy of judgement must be seen in relation to another: whether low size-constancy distorts reality more and is less normal than a similar degree of over-constancy, and whether under-estimation of distance is more pathological than over-estimation. Not enough work appears to have been done on distance judgement to allow one to attempt an answer. For size-constancy, however, there is some evidence that under-constancy is more common in younger children than in adults, so that a trend away from under-constancy should characterize the mature individual (sex differences are also known to be present, further complicating a difficult issue). Since the development of size constancy undoubtedly contains a considerable learning factor, a general hypothesis linking over-constancy and perceptual over-learning may be worth testing. Some evidence for such a relationship may be read into the present finding that the smokers of the non-psychotic group showed significant over-constancy for the Packet of Cigarettes. Extension of this line of reasoning would lead to the reverse hypothesis that under-constancy in schizophrenia may be due to under- or faulty perceptual learning, a position which formed one of the predictive bases of this study.

The present results have certainly shown that there may be other and better methods by which cue responsiveness could be more analytically investigated. One approach could be in terms of gradually increasing the amount of cues present in the total perceptual field either by progressively increasing illumination or by interposing gradually between standard and variable a series of secondary cues from other and familiar objects. Alternatively, experiments might be designed to elucidate for a start those objects and situations to which chronic psychotic patients would be most responsive, i.e. those having high motivational value. Such objects, and objects having low motivational value, could then be presented for size judgement with or without the kind of extraneous stimulation which Tizard & Venables (1957) have shown to increase the general responsiveness of withdrawn schizophrenics in terms of reaction times. In future it would certainly be necessary to control objectively for a variety of cognitive, clinical and skill factors which may be of equal or greater importance in general stimulus responsiveness and size and distance judgements than age, chronicity, intelligence or psychotic sub-classification.

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EFFECT OF VISUAL SPATIAL CONTEXT ON PERCEPTION OF ONE'S OWN BODY

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Three experiments, dealing with the effect of extension of surrounding visual spatial context on apparent size of body parts, were conducted. In the first two studies (forty-eight subjects in each) the dependent variable was arm length, and in the third study (twenty-four subjects) the dependent variable was head width. It was found that apparent arm length and apparent head width are relatively larger in an 'open-extended' visual spatial context than in a 'close-confined' spatial context. These results are interpreted in keeping with developmental and sensory-tonic theory.

I. INTRODUCTION

The present study is part of an experimental programme devoted to the analysis of the perceptual properties of one's own body from the viewpoint of sensory-tonic and developmental theory (Wapner & Werner, 1957). In regard to this analysis a number of studies have been conducted dealing with size perception. Evidence is available that articulation of the body boundary, e.g. through touching it, makes for shrinkage of apparent size of body parts, viz. the head (Werner, Wapner & Comalli, 1957; Wapner, Werner & Comalli, 1958) and arm length (Humphries, 1959; McFarland, Wapner & Werner, 1960). In one study (Wapner, 1960) it has been found that apparent head size is greatest in young children and decreases with increase in age; in another study it has been shown that under lysergic acid diethylamide (LSD-25), assumed to induce object self-dedifferentiation, there is an increase in apparent head size and apparent arm length (Liebert, Werner & Wapner, 1958). These studies provide support for the hypothesis that with greater differentiation between one's body and environment there is a shrinkage of apparent size of body parts, and vice versa.

The dependency of body perception on the relation between body and environment is investigated further in the present study by varying the extension of the surrounding visual spatial context. It is hypothesized that as the extension of the visual spatial context is varied differentially in relation to parts of the body, there are parallel changes in apparent size of body part. The notion underlying this hypothesis is that openness or closeness of surrounding space makes for experienced changes in degree of confinement, which in turn affects perception of body size.

Three studies dealing with effect of size of the visual spatial context on apparent size of body parts were conducted to explore this hypothesis. The dependent variable in the first two studies is apparent arm length and in the third study the dependent variable is apparent head width.

II. EXPERIMENT 1. EFFECT OF VISUAL SPATIAL CONTEXT ON APPARENT ARM LENGTH

In this experiment the specific hypothesis tested was that the outstretched arm is perceived as relatively longer in an 'open-extended' spatial context as compared with a 'close-confined' spatial context.

(a) *Procedure*

The task for the subject was to stand erect with fingers and arms outstretched at shoulder height, palms down, and in this position to judge which arm appeared longer or whether they appeared equal in length. This task was carried out while the subject was blindfolded, following observation of his own location in each of four visual spatial contexts.

The spatial contexts were provided by two of the psychology laboratory hallways which are arranged at right angles to one another. The test conditions are shown diagrammatically in Fig. 1. In test condition *A*, the subject was positioned so that the wall of the hallway with 'open-extended' space was aligned with his objective median plane, i.e. his left arm was outstretched toward the 'open-extended' space and his right arm was outstretched so that his finger tips were 4 in. from the wall of the main hallway, which was fronto-parallel to the subject. In condition B, both arms were outstretched toward 'open-extended' space. In condition C, both arms were outstretched with finger tips 4 in. from the wall-barrier; and in condition D, the subject was positioned in parallel fashion to condition A, but in such a manner that his right arm was outstretched toward 'open-extended' space and his left arm was outstretched with finger tips 4 in. from the 'wall-barrier' of the main hallway, which was located in the fronto-parallel plane. In all conditions the subject was positioned so that his finger tips were 4 in. from the plane of the main hallway.

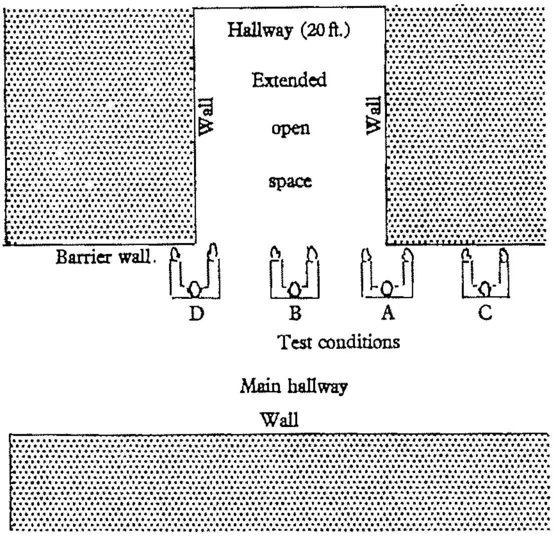

Fig. 1. Expt. 1 and 2. Diagrammatic sketch of test conditions.

The specific procedure for each text condition was as follows: while the subjects' eyes were closed he was positioned appropriate to the particular trial. Once the subject was positioned for the particular trial, he was asked to maintain the same body position while opening his eyes and inspecting the visual spatial context in which he was located. The subject was then required to report which arm was longer than the other, or whether they were equal in length. The trial was terminated once the subject made his report. He then walked around and was positioned for the next trial.

Forty-eight subjects, twenty-four men and twenty-four women, were tested under each of the four conditions. Conditions were systematically randomized with six men and six women in each of four sequences, ABCD, DABC, CDAB, BCDA. One trial was given for each test condition. The measures analysed were the reports of the subject, viz. 'left arm longer', 'right arm longer', or 'equal'.

(*b*) *Results*

The results are given in Table 1, which presents the frequency of each of the three reports for the four test conditions. When the right arm is outstretched toward 'open-extended' space, twenty-eight subjects report that the right arm appears longer and only eleven report that the left arm appears longer. In contrast, when the left arm is outstretched toward 'open-extended' space, thirty-seven subjects report that the left arm is longer and only six report that the right arm is longer. For both control conditions, open space in front of both arms, and wall-barriers in front of finger tips of both arms, the large majority of responses are 'equal' with approximately an even split between left and right arms perceived as longer. Thus, the arm outstretched toward visual 'open-extended' space is perceived as relatively longer than the arm which is outstretched towards a 'wall-barrier'.

Table 1. *Experiment* 1. *Frequency of reports concerning apparent arm length following exposure to different visual spatial contexts* ($N = 48$)

	Test conditions			
Report	Left arm: open space; right arm: barrier	Open space in front of both arms	Barrier wall 4 in. from both arms	Left arm: barrier; right arm: open space
Left arm longer	37	13	13	11
Arms equal	5	20	25	9
Right arm longer	6	15	10	28

Because of the lack of independence of data among the four conditions—the subject was used as his own control—the distribution of reports under the various test conditions was not tested by chi-square but a different test of significance, the sign test, was used. For this purpose the reports obtained for each subject on the two main conditions, A and D, were examined to determine whether the direction of results was in keeping with the hypothesis for each subject. This analysis revealed that of the forty-eight subjects tested the reports of thirty-one subjects were in keeping with the hypothesis, the reports of three subjects were contrary to the hypothesis, and the reports of fourteen subjects were equivocal. This distribution is significant at the 0·05 level of confidence. Thus, the evidence supports the hypothesis that the arm is perceived as relatively longer when it is outstretched toward 'open-extended' space than when it is outstretched toward relatively 'close-confined' space by virtue of a nearby wall-barrier.

III. EXPERIMENT 2. FURTHER TEST OF EFFECT OF VISUAL SPATIAL CONTEXT ON APPARENT ARM LENGTH

In the first experiment the test was carried out while the subject was positioned so that he was located *physically* at the specified distance from the barrier-wall or from the fronto-parallel plane in which it was located. It appeared possible that while the subject was positioned so that his arms were physically the same distance from the wall or its fronto-parallel plane, the subject might, because of positioning of his arm by the experimenter, experience inequality of arm length which in turn might influence the effect of the visual spatial context on apparent arm length. Accordingly, the procedure was varied. Here the subject was required to make minor adjustments of his arms himself until they appeared to be of equal length before the visual context conditions were introduced.

(a) *Procedure*

The same conditions were employed in this as in the previous experiment with one main difference. In the previous experiment the subject was positioned with finger tips 4 in. from the 'barrier-wall' or the fronto-parallel plane that this 'barrier-wall' of the main hallway defined. Here again, the subject was first positioned so that his arms were at a phyically set distance as in the previous experiment, and then, while his eyes were still closed, the subject was asked whether his arms appeared to be of the same length. If he reported that the arms appeared of equal length, the experiment was continued in the usual way. If, on the other hand, the subject reported that one arm appeared longer than the other, he was required, with eyes closed, to reposition himself until the two arms appeared as equal in length. Once this task had been carried out, the trial began as in the previous experiment. Here once again there was one trial under each of the same four conditions as described in Expt. 1 (see Fig. 1).

Forty-eight additional subjects, twenty-four men and twenty-four women, were tested under each of the four conditions. As before, conditions were systematically randomized with six men and six women in each of four sequences, ABCD, DABC, CDAB, BCDA. One trial was given for each test condition. The measures analysed, as in the previous experiment, were the subjects' reports of apparent arm length, 'left arm longer', 'right arm longer' or 'equal', following exposure to the visual spatial context defined by the particular test condition.

(b) *Results*

The results are presented in Table 2, which gives the frequency of reports, 'right arm longer', 'left arm longer' or 'equal' under the three test conditions. When the right arm is outstretched toward 'open-extended' space thirty subjects report that the right arm appears longer, and only thirteen report that the left arm appears longer.

Table 2. *Experiment 2. Frequency of reports concerning apparent arm length following exposure to different visual spatial contexts*($N = 48$)

	Test conditions			
Report	Left arm: open space; right arm: barrier	Open space in front of both arms	Barrier wall 4 in. from both arms	Left arm: barrier; right arm: open space
Left arm longer	28	14	16	13
Arms equal	8	15	16	5
Right arm longer	12	19	16	30

In contrast, when the left arm is outstretched toward 'open-extended' space, twenty-eight report that the left arm is longer and only twelve report that the right arm is longer. For both control conditions there is approximately an even split between reports of 'left arm' and 'right arm' perceived as longer, and a higher proportion of reports of 'equal' than in the experimental test conditions varying the spatial context for right as compared with left arm.

Analysis shows that of the forty-eight subjects tested, the reports of thirty subjects are in keeping with the hypothesis, the reports of ten subjects are contrary to the hypothesis, and the reports of eight subjects are equivocal. This distribution also is significant at the 0·05 level of confidence.

Taken together, the results of both experiments provide support for the hypothesis: when the arm is outstretched toward 'open-extended' space it appears longer than when it is outstretched toward a 'wall-barrier'.

IV. EXPERIMENT 3. EFFECT OF SURROUNDING SPATIAL CONTEXT ON APPARENT HEAD SIZE

The effect of different surrounding visual spatial contexts on body perception was examined in this experiment with respect to apparent head size.

(*a*) *Procedure*

In this experiment the task for the subject was, with eyes closed, to estimate the distance from his nose to his cheekbones, by means of pointing. The subject was required to indicate by pointing with right and left index fingers where he felt his cheekbones were projected on the fronto-parallel plane. The device for measurement (see Fig. 2) consisted of a metre stick mounted horizontally, 18 cm. from the subject's nose. The task was carried out while the subject's nose touched a rod-like projection mounted perpendicular to the scale; and he was positioned so that the rod-like projection was located in his median sagittal plane. The subject remained standing throughout a trial, kept his hands at his sides between trials, and was required to bring his finger tips *directly* up to the horizontal metre stick when indicating the apparent width of his head. Prior to the actual trial the subject opened his eyes, looked at the surrounding visual spatial context; he then closed his eyes and indicated his apparent head width.

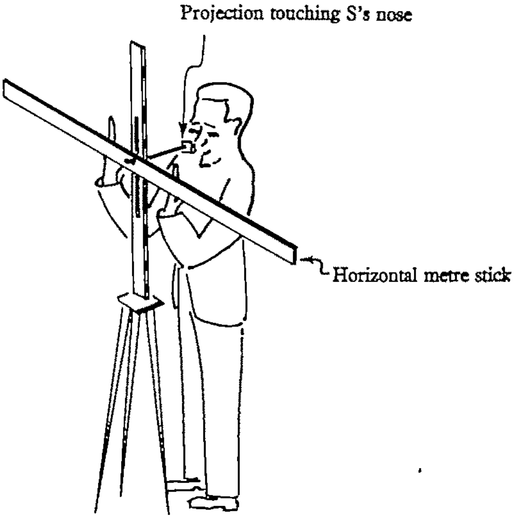

Fig. 2. Sketch of device for measurement of apparent head width.

The task was carried out while the subject was located in four different spatial contexts which he inspected prior to indicating head width: condition A, extended symmetrical space—the subject was placed in the centre of a room (21 × 12 ft.) so that his sides were equidistant from the walls at his left and right, i.e. his nose was located 10·5 ft. from the left and right walls; condition B, asymmetrical space extended on right—the subject was placed in the 21 ft. room so that his left side was next to the left wall, i.e. the distance from his nose to the left wall was 1 ft., whereas the distance from his nose to the right wall was 20 ft.; condition C, asymmetrical space extended on left—the subject was placed so that the distance from his nose to the right wall was one foot, and the distance from his nose to the left wall was 20 ft.; and condition D, confined symmetrical space—a space 2 ft. wide was partitioned off and the subject was placed so that his nose was one foot from the wall on his left and the wall on his right.

Twenty-four subjects, twelve men and twelve women, were tested in each of the four conditions. A 4 × 4 Latin square design with three men and three women in each sequence was employed.

(b) *Results*

The summary of the analysis of variance is presented in Table 3. It can be seen that Conditions is the only significant source of variance. In keeping with expectation, apparent head size is widest in the extended symmetrical space context (22·1 cm.), narrowest in the confined symmetrical space context (20·4 cm). and intermediate in the asymmetrical space contexts (20·7 cm.; 21·4 cm.). Under extended symmetrical space context mean apparent head size is significantly greater than under confined symmetrical space context and under asymmetrical space extended on right and confined on left. Thus, the results are in keeping with the hypothesis that following exposure to an open-extended context in the left–right dimension, apparent head width is greater than following exposure to a confining visual spatial context.

Table 3. *Experiment* 3. *Apparent head width following exposure to different visual spatial contexts*

Source	D.F.	Mean square	*F*	*P*
Individuals (I)	23	100·42		
Sex	1	45·10	< 1·00	—
Sequence	3	238·70	2·92	N.S.
I's within seq.	19	81·50		
Order	3	3·11	< 1·00	—
Conditions	3	14·10	3·58	< ·05
Square uniqueness	6	5·50	1·39	N.S.
Error	60	3·94		
Total	95	27·69		

Means and mean differences (cm.)

Test condition	Means	Mean differences			
		A	B	C	D
A. Extended-symmetrical	22·1	—	1·4*	0·7	1·7*
B. Extended to right: confined left	20·7	—	—	0·7	0·3
C. Extended to left: confined right	21·4	—	—	—	1·0
D. Confined-symmetrical	20·4	—	—	—	—

* Significant at 0·05 level.

V. DISCUSSION

The present experimental findings extend our knowledge concerning conditions under which there are changes in apparent size of body parts. As mentioned in the introduction, previous studies have brought out a number of factors which influence apparent size of body parts: articulation of the boundary of body parts (head and arm) makes for a relative diminution in apparent size of those body parts; apparent head width decreases from childhood to adulthood; and apparent head width and arm length increases in size under the influence of LSD. The present studies have demonstrated that as the extension of the surrounding spatial context is varied in relation to the position of the body, there are consonant changes in apparent size of body parts; apparent arm length and apparent head width are relatively greater in an 'open-extended' spatial context than in a 'close-confined' spatial context.

The aforementioned studies introduce experimental variables of various kinds. They involve: developmental changes due to age and regressive drugs; changes due to the degree of strength of boundary between body and environment; and—in the present study—changes due to the degree of confinement of the body in regard to the surrounding environment. Despite the variety of factors, they can be considered as having a common basis if one assumes that the experimental variables they introduce involve changes in the relationship between body and environment, which are then reflected in concomitant changes in size perception of one's own body.

Beyond stating the commonness of these factors in terms of the relationship between body and environment, there yet remains the need for an interpretation of the processes underlying the changes in body perception. This interpretation, in our view, can be handled best by an organismic theory—for us, sensory-tonic theory. It requires an examination of the relation obtaining between proximal stimuli and organismic state. We would maintain that for perception of one's own body, just as for perception of external objects, one must apply the fundamental postulate of sensory-tonic theory, viz. 'perception is a reflexion of a relation between state of the organism and proximal stimuli' (Werner & Wapner, 1952; Werner & Wapner, 1956; Wapner & Werner, 1957).

At the present state of our experimental knowledge we can only show the general direction that an interpretation in sensory-tonic terms would have to take. It will be advantageous in sketching the character of such an interpretation to consider first the manner in which sensory-tonic theory has dealt with perception of external objects. For that purpose we may consider one of our principal situations, object localization. The proximal stimulus—retinal impression—issuing from an object in a physically *fixed* position is interpreted differently depending on changes in the state of the organism, i.e. the apparent position of that physical object varies with changes in the state of the organism. To illustrate, consider apparent verticality under conditions of body erect, with and without unilateral stimulation of the sterno-cleido mastoidius neck muscle. Unilateral neck muscle stimulation is assumed to affect organismic state defined as the neuromuscular configuration emerging from active-counteractive processes (Wapner & Werner, 1957). Here, a physically perpendicular rod is perceived as tilted to the right when the right neck muscle is stimulated, and tilted to the left when the left neck muscle is stimulated (Wapner, Werner & Chandler, 1951). Thus, with the same proximal stimulus, the perception varies depending on the state of the organism.

A relationship parallel to that for object perception obtains with regard to perception of one's own body. Consider the perception of one's own position in space in terms of the longitudinal axis of one's body (McFarland, Clarkson & Wapner, 1961) under conditions of body tilt. Here too, the apparent position of the longitudinal body axis can be interpreted in terms of a relation between proximal stimulus and organismic state. Proximal stimuli relevant to the longitudinal axis of the body are those stimuli coming from the body (tactual, kinaesthetic, interoceptive, etc.) which define the body *per se*, i.e. independent of position; organismic state, again, refers to the neuromuscular configuration emerging from active-counteractive processes. Given the same proximal stimulus pertinent to body axis, with a change in organismic state through body tilt, there is a change in perception of the position of the body axis. This means that we would not perceive the body as tilted if we were not able to relate the body axis to the changed organismic state.

We may now apply analogous reasoning from properties of the body in terms of position to properties of the body in terms of size. Considering head size, again there are proximal stimuli with respect to the surface of the head *per se*, which are related to the state of the organism; then, again, apparent head size is a reflexion of this relationship. Here, too, organismic state is conceived in terms of neuromuscular configuration, though we are not in a position at this time to specify the processes involved. However, we do feel justified at least in hypothesizing that as one's body is put into different extension contexts (small *vs.* large surrounding space) there are concomitant changes in organismic state. In contradistinction to perception of position, where the pertinent aspect of organismic states relates to left-right symmetry and asymmetry, here, the pertinent aspect of organismic state relates to extensionality conceived in terms of the polarity 'expansiveness-constrictiveness'. For example, we would argue that one experiences the body as 'expansive' when adopting the posture of stretching; and in contrast that one experiences the body as 'constricted' when taking on the posture of crouching. We assume then that the same proximal stimulus from the body *qua* object is perceptually 'interpreted' differently under 'expansive' *vs.* 'constrictive' organismic states.

The present state of our knowledge permits us to interpret size perception of one's body parts only in these general vectorial terms. Before elaborating further it will be necessary to accumulate a variety of facts concerning body size perception under changes of organismic state, such as voluntary stretching or constricting of one's body, changes of body state due to the degree of restrictiveness in a surrounding medium, etc. Moreover, in order to develop a comprehensive theory of size perception it will be necessary to deal not only with perception of the body, but also to include analogous studies on size perception of external objects.

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THE VALIDITY OF QUESTIONNAIRE AND RATING ASSESSMENTS OF EXTRAVERSION AND NEUROTICISM, AND THEIR FACTORIAL STABILITY

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Groups of nominated extraverts and introverts, and of stable and neurotic subjects were given a 188-item questionnaire containing questions thought to be relevant to the measurement of these two dimensions of personality. Factor analyses were also carried out for 124 of these items making available factor loadings on these two dimensions. An analysis was carried out of the relationship between rated and self-rated behaviour, and it was concluded that as far as extraversion was concerned, there was considerable agreement between these two methods of assessment. As regards neuroticism, agreement was much less close, and a theoretical explanation of this fact is given, supported by a special analysis of the empirical data. It was found that the factor analytic method of locating the E and N factors had considerable stability as from one study to another, in spite of considerable differences between the studies in choice of sample, choice of items, and other factors.

1. INTRODUCTION

The vast majority of studies in the personality field use either ratings or self-ratings (questionnaires) as the criterion, although the validity of these criteria has never been very firmly established. After reviewing the evidence, Eysenck (1960*a*) concluded that while both methods were subject to possible errors of various kinds, these errors were different for the two methods; agreement between them could therefore reasonably be regarded as evidence of validity for both. Disagreement between the two methods cannot be interpreted very easily in the absence of further evidence, as it might be due to lack of validity in either the questionnaire responses, or the ratings, or both. Considering the importance of this issue, it is remarkable how little empirical evidence is available, particularly in the non-psychiatric field. The experiment here reported constitutes an attempt to provide some evidence on this point, with particular reference to the two personality dimensions of extraversion and neuroticism. An effort has also been made to study the stability of these two factors by comparing different factor analyses using the same items.

2. METHOD OF STUDY

The method here used, one of several discussed by Eysenck (1954), is called the *method of nominated groups*; it has been used previously by S. B. G. Eysenck (1962) in an attempt to validate the M.P.I. (Eysenck, 1959) against a rating criterion. The procedure used was as follows. Members of the Psychology Department in the Institute of Psychiatry were asked to nominate friends or acquaintances, known to them for some while, who seemed to them to be oustandingly extreme on either the *extraversion* or *neuroticism* dimension, or both. In other words, they were asked to pick people on the basis of their *behaviour*, if this seemed to them to be extremely extra-

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* We are indebted to the Research Fund of the Maudsley and Bethlem Royal Hospitals for the support of this investigation.

verted, extremely introverted, extremely stable, or extremely neurotic. Some judges chose candidates who seemed to them to be high on both dimensions. To guide the judges, the following definition of extreme extraversion and introversion was given them:

> The typical extravert is sociable, likes parties, has many friends, needs to have people to talk to, and does not like reading or studying by himself. He craves excitement, takes chances, often sticks his neck out, acts on the spur of the moment, and is generally an impulsive individual. He is fond of practical jokes, always has a ready answer, and generally likes change; he is carefree, easygoing, optimistic, and likes to 'laugh and be merry'. He prefers to keep moving and doing things, tends to be aggressive and lose his temper quickly; altogether his feelings are not kept under tight control, and he is not always a reliable person.
>
> The typical introvert is a quite, retiring sort of person, introspective, fond of books rather than people; he is reserved and distant except to intimate friends. He tends to plan ahead, 'looks before he leaps', and distrusts the impulse of the moment. He does not like excitement, takes matters of everyday life with proper seriousness, and likes a well-ordered mode of life. He keeps his feelings under close control, seldom behaves in an aggressive manner, and does not lose his temper easily. He is reliable, somewhat pessimistic, and places great value on ethical standards.

For neuroticism, it was suggested they nominate people who seemed to them to behave as if they had a large number of neurotic symptoms, and whom they would expect to break down fairly easily given some degrees of stress. On the other hand, the stable group required was to contain people who seemed so non-neurotic to the judge concerned that only the very greatest stress would produce neurotic symptoms and behaviour or a tendency to break down.

The judges were asked to make sure that the subjects were neither psychologists, nor married to psychologists, and that their English was adequate to answer the many questions contained in the inventory. The subjects thus nominated were then asked, by the judges, to fill in a questionnaire of 170 items thought to be relevant to extraversion or neuroticism (including the 48 items of the M.P.I.); there were also 18 'Lie Scale' items which are of no relevance in this connexion. Completed questionnaires were sent directly to the authors by the subjects, and were not seen by the judges. There were 25 subjects in each of the four groups: E, I, S and N (S, stable; N, neurotic; E, extravert; and I, introvert). The total number of subjects was 100, but several were allocated to more than one group by the judges, of whom there were 22.

The identification of an item as being diagnostic of extraversion, introversion, stability or neuroticism cannot of course be undertaken on a purely subjective basis. For 124 items, factor loadings on E and N were available from a factor analysis which had been undertaken on a separate group of 300 subjects. The essential comparison in this study will be of the factor loadings of any given item with the degree to which that item differentiates between the E and I, or the S and N nominated groups. This differentiating property of the item will be designated the D (difference) score; it is calculated by subtracting the number of endorsements by nominated introverts from that given by nominated extraverts (D_E), and by subtracting the number of endorsements by nominated stable subjects from that given by nominated neurotic subjects (D_N). The main hypothesis under examination predicts that items with high D_E scores will also have high loadings on the E factor, while items with high D_N loadings will have high loadings on the N factor. (Both loadings and D scores can be positive or negative, of course, and in the above sentence the word 'high' is understood as meaning 'high positive'; a high negative value would be called 'low'. This convention will be followed throughout this paper.)

3. FACTOR ANALYSIS

The 124 items used in the analysis were selected from the total number in the questionnaire on *a priori* grounds, and are given in Appendix Table 1. They were divided into two groups, as the electronic computer was unable to accept the total set. The 300 subjects were University and evening class students, 140 male and 160 female. In addition to the 62 personality questions there was also included in each matrix of intercorrelations scores for age and sex, the number of '?' responses, and the 'Lie' scale score. Product moment correlations were run between the 68 variables in each case, and a principal components analysis performed; three factors were taken out in each of the two analyses. Rotation was undertaken in accordance with Thurstone's principle of simple structure, and the first two factors were clearly identified as neuroticism and extraversion; loadings on these two factors for the 124 items are given in Appendix Table 1. (Evidence for this identification will be given below.) The third factor had high and almost identical loadings in the two analyses for 'Age' (0·511 and 0·491) and for the 'Lie' scale score (0·430 and 0·441). The 'Lie' score also had negative loadings on Neuroticism (−0·226 and −0·217). The number of '?' responses had negative loadings on Extraversion (−0·135 and −0·186). Loadings on sex for E and N are quite low, being 0·182 and 0·162 in the first analysis, −0·229 and 0·145 in the second (maleness being scored 1 point, femaleness 0 point). The third factor, with its high loadings on age and lie scores is not of great psychological interest, and will not be discussed further. The other two factors suggest a distribution of traits in the two-dimensional E–N universe rather along the lines indicated in Fig. 1; the similarities between such a picture and that of the old Galen–Kant–Wundt theory have been pointed out by Eysenck (1960*a*). In making up this figure, we have drawn mainly on the results of this study, but have also taken into account other published (Eysenck, 1956, 1959, 1962*a*, *b*; Eysenck & Eysenck, 1962) and unpublished work.

Our identification of the E and N factors may be questioned. The first line of evidence, of course, lies in the similarity of the items defining each factor with the trait-names used in describing the types. More objective, perhaps, is the evidence presented in the next section, relating to the responses of the nominated groups. A third line of evidence relates to the similar pattern of factor loadings emerging from the present study as compared with the original factor analysis of the M.P.I. items (Eysenck, 1956). If we look at the N items in this original study, we find that none of them has loadings on the E factor in the present study of more than 0·25; all have loadings on the N factor of between 0·25 and 0·58, with the median in the 0·45–0·49 interval. Looking at the E items of the original study, we find that only 3 have N loadings in excess of 0·25 (none as high as 0·35). E loadings range from one exceptionally low one of 0·13 to 0·62, with the median in the 0·40–0·44 interval. No item is in the wrong quadrant. These results suggest substantial similarity between the two studies, although the samples of subjects were quite different, and although the matrix of items in which the M.P.I. items were embedded in the present study was not used at all in the original work. For the new items, i.e. those not contained in the original M.P.I., a prediction was made in each case as to the expected position of the item in the two-dimensional framework of the N and E factors; nearly all of these predictions were in fact borne out, although not always at acceptable levels of significance. Lengthy discussion of these predictions would not seem necessary as they are all rather obvious, can mostly be deduced from the general theory of extraversion and neuroticism (Eysenck, 1957), and have indeed been discussed at various times (Eysenck, 1960*a*, *b*).

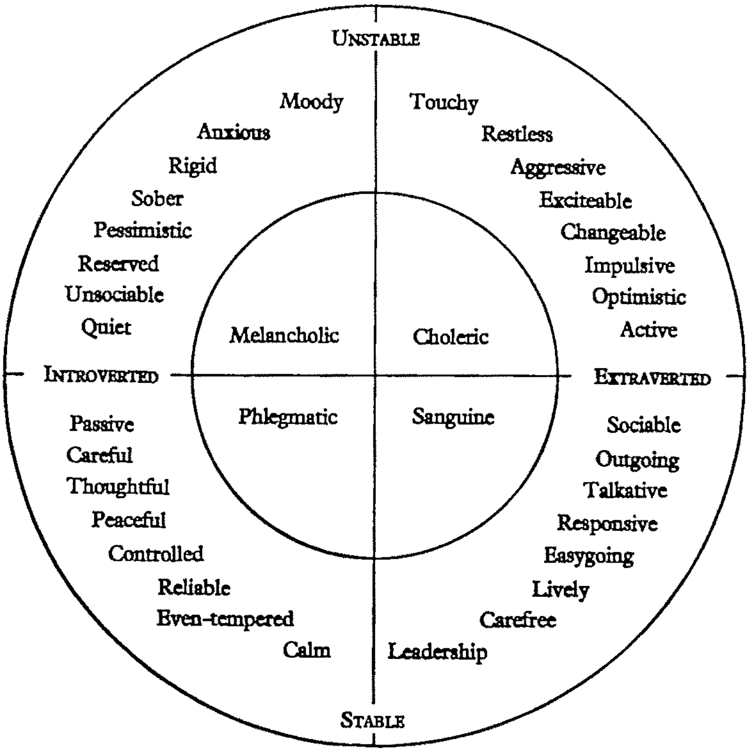

Fig. 1. Diagram showing approximate position of various traits in two-dimensional factor space. Also shown are the four classical 'temperaments' or 'humours', corresponding to the four quadrants.

4. RESULTS

The results of the validation study are given in Appendix Table 1, which lists the items on which factor loadings were available, D_E and D_N scores, and E and N factor loadings. These detailed results, to which reference will be made in the discussion, can be summarized conveniently in the form of four correlations. D_E correlates 0·883 with the E loadings, and $-0·270$ with the N loadings; D_N correlates 0·676 with the N loading, and $-0·525$ with the E loading. (Correlations of 0·17 and 0·23 are significant at the 5 and 1 % levels respectively for N = 124.) We thus find that in both cases the predicted positive relation between D_E and E loading, and D_N and N loading, is indeed observed, assuming a very high value in the former case; in addition, however, there is also a less welcome correlation of considerable magnitude between D_N and the E loading (and to a much lesser extent between D_E and the N loading.) These require some detailed discussion.

5. DISCUSSION

It is possible to formulate an hypothesis to account for the apparent tendency of judges to choose their N and S groups in such a way that these two groups are also differentiated in terms of introverted and extraverted questionnaire answers. Such an hypothesis may suitably start with a finding reported by Eysenck (1956) regarding the allegedly unitary personality trait of sociability (Guilford & Guilford, 1936). Using Guilford scales R and C respectively as measures of extraversion and neuroticism, a procedure justified on empirical grounds elsewhere (Eysenck, 1960*a*) he proceeded to test the hypothesis that items in Guilford's S (social shyness) scale would break up into two groups—introverted shyness and neurotic shyness. Put briefly, this hypothesis suggested that 'the introvert does not *care* to be with other people; the neurotic is *afraid* of being with other people' (Eysenck, 1956, p. 121). Two analyses were done, using groups of 200 men and 200 women; for the men it was found 'that *none* of the items showing a significant relation with R shows a significant relation with C. Similarly, not one of the items showing a significant relation with C shows a significant relation with R. With the exception of one item the same is true for the women'. A detailed analysis of the items revealed that 'the sociable extravert lets himself go and has a hilarious time, likes to mix socially,...is a good mixer who does not stay in the background on social occasions, who takes the initiative in making friends, has many social engagements...etc. In other words, he is a person who enjoys social intercourse with people as opposed to the introvert, who does not enjoy social intercourse with people. When we turn to the items indicative of neurotic social shyness we find the shy person troubled about being self-conscious, experiencing periods of loneliness, troubled with feelings of inferiority and self-conscious with superiors, worrying about humiliating experiences, and about being shy, ill at ease with other people, and not well poised in social contacts. In other words, we meet a kind of person who is troubled and worried over his social contacts, and would like to be more adequate in his dealings with other people, but whose emotional reactions seem to interfere with his social adjustment' (p. 124).

These two types of social shyness are of course easily differentiated by introspection, and consequently accessible to questionnaire probing, but they are confounded when we apply a simple behavioural criterion, as both lead to non-social behaviour, however different the underlying motivation. Thus to the observer social shyness will often appear to be associated with neuroticism, as well as with introversion, and he will be unable in most cases to distinguish 'introverted shyness' from 'neurotic shyness'. Thus our judges, in choosing the N and S groups, might have been expected to have based their choice partly on overt lack of sociability in the candidates they selected as members of the N group. It is likely that in doing this they selected erroneously a number of unsociable introverts. For these individuals the D_N scores should be high on 'introverted social shyness' items; at the same time these items would have high negative loadings on extraversion.

Let us now look at the items in the questionnaire having loadings of $-0{\cdot}35$ or more on extraversion, and D_N scores of 10 or higher. There are 8 such items: they deal with keeping in the background on social occasions, being quiet in a social group, being shy with persons of the opposite sex, limiting one's acquaintance to a select few, having difficulties in 'losing oneself' at a lively party, being remote and distant except with intimate friends, being naturally reserved, and tending towards pessimism. With the exception of the last item (which only just qualified for inclusion in this group) all are indeed, as expected, 'social shyness' items of the introverted kind, with uniformly low neuroticism loadings (all are below 0·27) and uniformly high D_E scores (ranging from 17 to 28). In the opposite quadrant (high E loading, low D_N scores) there are only 4 items if we use the same criterion as before, to which may perhaps be added another 4 which are only slightly outside the area specified. These 8 items deal with preferring rapid action, being (and being regarded as) lively, being able to have a good time at a party, liking to mix with people socially, being happy-go-lucky, being talkative, and keeping in close touch with things around one. These items are rather more mixed, only 3 obviously belonging to the 'sociable extraversion' kind. On the whole, however, the data seem to bear out the hypothesis quite well.

If this conclusion is acceptable, it follows that the discrepancies between ratings and self-ratings are in effect more likely to argue against the acceptance of the ratings, as these confound two possible causal determinants of observed 'sociable' and 'unsociable' behaviour. Thus the results of this experiment suggest unambiguously that as far as *extraversion* is concerned, self-ratings and behaviour as rated by others agree well; as far as neuroticism is concerned, the picture is rather less clear, but if our argument be accepted, then we may regard the self-ratings as valid, and the ratings as rather less so. This conclusion is in good agreement with the theory put forward by S. B. G. Eysenck (1962) that 'extraverted behaviour is more easily observable by the outsider, whereas neuroticism is more characterized by subjective internal conditions, such as anxiety and other conditions, which may not give rise to observable differences in behaviour'. It may finally be suggested that the method of nominated groups may be of considerable use in analysing problems of validity in this field.

A final word may be permitted regarding the results of this and previous factor analyses, as diagrammed in Fig. 1. Eysenck (1960*a*) has elsewhere drawn attention to the curious fact that different investigators, such as Guilford and Cattell, starting with a universe of items not very different one from the other, arrive at some 16 or so 'primary factors' which are in no way similar; when these factors themselves are correlated (within each system) and factor analyses are performed, however, both solutions give rise to very clear-cut neuroticism and extraversion factors. The tentative solution to this problem given in the earlier publication may be illustrated by reference to the alleged personality trait (first-order factor) of 'rigidity'. When this was investigated in detail, it appeared that self-rated 'rigid' behaviour was entirely a function of introversion, neuroticism, and low intelligence (Eysenck & Eysenck, 1962): 'rigidity as a trait independent of these second-order concepts has no existence or statistical meaning'. It is theoretically possible too chose any small sector of Fig. 1, sample the universe of questions in this sector, and arrive at a factor or trait which can be named and which makes obvious 'sense' in terms of everyday behaviour and language. It is certainly possible to attribute the observed correlations to such a hypothetical trait; it would, however, be equally possible, and more parsimonious, to attribute them to the fact that points in a particular circumscribed region of the diagram have similar loadings on our two reference factors. It would follow from this second hypothesis that by suitable sampling (i.e. by slight shifts of the sector chosen for investigation) one would be enabled to extract an infinite set of first-order factors or 'traits' from the whole universe of questions; this indeed appears to have happened in the sets of investigations carried out by Guilford and Cattell. It seems likely that slight differences in item sampling are responsible for the emergence of distinct sets of traits from their otherwise closely comparable investigations. In spite of such disagreement in the field of first-order factors, however, it would follow from our hypothesis that second order factors should be closely similar, and should also be similar to the N and E factors as measured by the M.P.I. Other empirical findings which can be explained in terms of such a conception are those of Peterson (1960) that when a correlational test of factor similarity was applied to the results of Cattell on the factorial structure of personality in children and adults, much greater similarity of structure was observed when the analysis was carried out in terms of E and N than when Cattell's set of primary factors was used.

It is not denied, of course, that after the extraction of E and N from a matrix there may remain clusters of correlations identifying traits which have objective existence over and above the communality achieved through the fact of similar E and N loadings. What is suggested is rather that the existence of such clusters must remain an empirical problem which cannot be solved by simply assuming their existence; in our experience the matrices of residual correlations after the extraction of E and N tend to be remarkably similar to matrices entirely due to chance factors. This may in part be due to our endeavour to sample items relevant to the E and N factors, and it is not considered that the existence of further factors is in any sense ruled out by our findings.

There is, however, another consideration which must not be neglected; this relates to the psychological status, as opposed to the statistical status, of first- and second-order factors in this field. We would maintain that personality traits are best regarded as 'habits', thus reducing these imperfectly understood concepts to a more basic and better understood type of variable. Habits are typically evanescent, changing, and difficult to classify, and traits are notorious in presenting similar difficulties. However, E and N may be regarded as being at an altogether more fundamental level. Eysenck (1957, 1960*a*) has argued in favour of constitutional differences in the formation of excitatory and inhibitory potentials as being the underlying cause of introverted and extraverted behaviour patterns, and of constitutional differences in autonomic lability as being the underlying cause of neurotic and stable behaviour, and it would seem to follow that E and N have a different conceptual status in psychology from that achieved by the shifting and purely descriptive 'traits' identified by factor analysis. It is of course realized that these constitutional factors interact with environmental events to produce the final habit family hierarchies which constitute personality

(Eysenck, 1960*c*), but nevertheless they must be assumed to exert a powerful and possibly decisive influence on behaviour. Without some such theoretical referent outside the closed circle of correlational analysis, it would clearly be impossible to break out of this circle and relate statistical factors in any meaningful way to psychological theory generally, and experimental psychology specifically. Fig. 1 illustrates fairly clearly that the hope of finding an *infallible* criterion in 'simple structure' must end in disappointment; every part of the plane generated by the E and N vectors is 'inhabited', as it were, and it follows from our theory that this should be so. In this, personality description (in terms of 'traits') differs, perhaps fundamentally, from the analysis of ability. It is not impossible, however, that purer measures of E and N, such as those developed by Eysenck (1957, 1960*a*; 1962*c*) may define these two factors much more clearly, and make possible the use of Thurstone's criteria of rotation.

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Appendix Table 1

The 124 items on which factor loadings were already available out of the 170 items in the questionnaire completed by the nominated groups.

E, N, extraversion and introversion loadings from a separate group of 300 subjects.
D_E, no. of endorsements by nominated extraverts *less* endorsements by nominated introverts.
D_N, no. of endorsements by nominated neurotics *less* endorsements by nominated stable subjects.
N = 100 nominated subjects, in four groups of 25.

	Loading			
	E	N	D_E	D_N
1. Do you sometimes say the first thing that comes into your head?	0·244	0·183	15	0
2. Can you usually solve a problem better by studying it alone than by discussing it with others?	−0·252	0·004	−21	10
3. In a group, do you hate having to introduce people to each other?	−0·339	0·366	−16	11
4. Do you very much enjoy good food?	0·442	−0·060	1	−5
5. Are you a person who is not much given to cracking jokes and telling stories to your friends?	−0·363	0·035	−20	9
6. Do you often crave excitement?	0·450	0·240	19	18
7. Do you frequently forget people's names and phone numbers?	−0·190	0·165	−5	−4
8. Can you put your thoughts into words quickly?	0·138	−0·347	11	−5
9. Do you enjoy practical jokes?	0·282	−0·045	4	5
10. Do you find it hard to tell anyone about yourself?	−0·206	0·246	−16	10
11. Would you say you are the type to fall in and out of love easily?	0·307	0·206	5	7
12. Do you have to be careful to keep from being too aggressive or domineering?	0·253	0·138	5	−2
13. Do you mostly succeed in keeping the expression of your feelings under very good control?	−0·107	−0·127	2	−7
14. Would you rate yourself as an impulsive individual?	0·501	0·293	26	2
15. Do your friends regard you as very reliable, i.e. likely to keep appointments, not forget errands, etc?	−0·125	−0·192	−1	−2
16. When you are drawn into a quarrel, do you prefer to 'have it out' to being silent hoping things will blow over?	0·242	0·060	6	3
17. Do you mind selling things, or soliciting funds for a cause in which you are interested?	−0·217	0·085	−16	5
18. Do you like to be in a situation with plenty of excitement and bustle?	0·475	−0·010	31	−9
19. If you want to learn about something, would you rather do it by reading a book on the subject than by discussion?	−0·240	0·036	−15	12
20. Do you often act on the spur of the moment without stopping to think?	0·521	0·404	20	4
21. Are you reserved and distant except to intimate friends?	−0·479	0·183	−25	14
22. When the odds are against your succeeding in some enterprise, do you think it worth while to take a chance?	0·333	0·046	8	0
23. Can you readily get some life into a rather dull party?	0·484	−0·130	27	−4
24. If you were among several witnesses to an accident, would you be likely to take charge?	0·141	−0·276	0	−4
25. Do you sometimes quite enjoy doing dangerous things?	0·190	0·030	14	−4
26. Do you like to have constant change in the kind of work you do?	0·071	0·021	9	−1
27. Are you ordinarily a carefree individual?	0·513	−0·259	18	−16
28. Do you prefer people who keep an open and hesitant mind for a long time to those who know at once exactly where they stand on issues (and even jump to conclusions)?	−0·183	0·164	−5	17

Appendix Table (*cont.*)

		Loading			
		E	N	D_E	D_N
29.	Do you sometimes sulk?	0·047	0·414	−3	0
30.	Do you find it very difficult to take no for an answer even when it is obviously impossible to do what you want?	0·192	0·393	8	4
31.	Do you like working alone?	−0·497	−0·097	−13	6
32.	Do you enjoy opportunities for conversation so that you rarely miss a chance of talking to a stranger?	0·366	−0·006	23	9
33.	Do you have difficulty in falling asleep easily at bedtime?	−0·353	0·199	−5	3
34.	Do you like to try fancy dishes?	0·295	−0·130	10	−8
35.	Are you inclined to stop and think things over before acting?	−0·443	−0·192	−17	6
36.	Do you tend to be slow and deliberate in movement?	−0·338	−0·002	−9	1
37.	Do you often need cheerful, sympathetic company to cheer you up?	−0·054	0·458	5	25
38.	Do you feel it essential to plan ahead carefully before beginning any undertaking?	−0·405	−0·027	−4	0
39.	Have you the tendency to elaborate and exaggerate an event when talking to friends about it afterwards?	0·122	0·162	6	2
40.	On the whole, do you prefer the company of books to people?	−0·393	0·040	−9	−1
41.	Would you hesitate to complain to a waiter or the manager if you are served bad food in a restaurant?	−0·178	0·330	−14	2
42.	Do you usually keep in close touch with things going on around you?	0·292	−0·192	7	−16
43.	Do you get tired easily when you are doing a repetitive job?	0·179	0·159	12	−2
44.	Do you hate being with a crowd who play jokes on one another?	−0·327	0·012	−2	4
45.	If you are annoyed by something, do you find it absolutely necessary to talk to somebody to 'let off steam'?	0·365	0·330	20	2
46.	Do you dislike doing more than one thing at a time?	−0·102	0·284	−12	14
47.	Is your motto to take matters of everyday life with proper seriousness rather than to 'laugh and be merry'?	−0·451	0·116	−24	0
48.	Would you rate yourself as a hardy type of person?	0·195	−0·162	11	−10
49.	Do you prefer a well-ordered mode of life with regular hours and an established routine?	−0·065	0·026	−8	17
50.	When arguing do you tend to raise your voice?	0·332	0·287	16	−2
51.	Do you tend towards an over-cautious pessimism?	−0·382	0·255	−17	13
52.	Do you often have a restless feeling that you want something but do not know what?	0·151	0·552	8	9
53.	Would you describe yourself as an easy-going person not concerned to be precise?	0·340	−0·087	12	5
54.	Do you prefer romantic stories to adventure stories?	0·035	0·265	2	5
55.	Do you find that your interests tend to change rather rapidly?	0·282	0·234	11	2
56.	Would you feel uncomfortable in anything other than fairly conventional dress?	−0·043	0·212	−3	9
57.	Are you a very ticklish person?	0·144	0·202	15	−1
58.	Do you do many things that make you feel remorseful afterwards?	0·206	0·518	−5	5
59.	Do you tend towards a rather reckless optimism?	0·513	0·115	25	−3
60.	Would you do almost anything for a dare?	0·452	0·015	15	−4
61.	Do you usually avoid 'sticking your neck out'?	−0·135	0·189	−9	9
62.	Do you go about your business rushing actively from one thing to another, e.g. eating fast, walking fast, etc.?	0·103	0·142	9	−2

Appendix Table (*cont.*)

	Loading			
	E	N	D_E	D_N
63. Do you prefer to dress soberly and correctly rather with a 'bit of a splash' that will make people take notice?	−0·144	0·108	−14	5
64. Do you like to keep moving around and doing things most of the time?	0·223	0·056	10	−5
65. When people shout at you, do you shout back?	0·376	0·247	0	−2
66. Would you rather have a job with a fixed secure and modest salary than one with a larger salary but irregular earnings depending on luck and your enterprise in making contacts?	−0·066	0·106	−15	12
67. Do you lose your temper easily, but get over it quickly?	0·205	0·291	10	−2
68. Other things being equal, would you prefer the job of a farmer to that of a life insurance salesman?	−0·342	−0·64	−14	0
69. Are you given to acting on impulses of the moment which later land you in difficulties?	0·458	0·474	28	2
70. Are you often annoyed by being temporarily prevented from doing something?	0·220	0·331	7	13
71. Do you sometimes slam doors when you are angry?	0·287	0·265	−4	16
72. Would you rather spend an evening by yourself than go to a dull party?	−0·383	0·041	−14	−1
73. Do you, at times, feel like picking a fight with someone?	0·234	0·245	−1	7
74. Do you often feel that you must discuss something you have read before you will really understand or remember it?	0·363	0·223	5	2
75. Does your natural reserve generally stand in your way when you want to start a conversation with an attractive stranger of the opposite sex?	−0·516	0·207	−25	16
76. Do you often sing or hum when you are by yourself?	0·254	0·267	5	2
77. Are you happiest when you get involved in some project that calls for rapid action?	0·397	−0·123	26	−10
78. Do you sometimes feel happy, sometimes depressed, without any apparent reason?	0·115	0·584	7	7
79. Does your mind often wander while you are trying to concentrate?	0·165	0·460	14	5
80. Do you usually take the initiative in making new friends?	0·543	−0·140	28	−5
81. Are you inclined to be quick and sure in your actions?	0·224	−0·178	23	−11
82. Are you frequently 'lost in thought' even when supposed to be taking part in a conversation?	−0·153	0·378	−2	12
83. Are you sometimes bubbling over with energy and sometimes very sluggish?	0·190	0·501	5	16
84. Would you rate yourself as a lively individual?	0·555	−0·230	24	−11
85. Would you be very unhappy if you were prevented from making numerous social contacts?	0·474	0·123	23	3
86. Are you inclined to be moody?	−0·118	0·534	−8	15
87. Do you have frequent ups and downs in mood, either with or without apparent cause?	−0·132	0·554	2	10
88. Do you prefer action to planning for action?	0·382	0·084	17	−5
89. Are your daydreams frequently about things that can never come true?	0·003	0·458	4	9
90. Are you inclined to keep in the background on social occasions?	−0·621	0·223	−25	26
91. Are you inclined to ponder over your past?	−0·164	0·415	−3	28
92. Is it difficult to 'lose yourself' even at a lively party?	−0·372	0·193	−22	15
93. Do you ever feel 'just miserable' for no good reason at all?	0·132	0·557	−2	11
94. Are you inclined to be over-conscientious?	−0·244	0·274	−9	11
95. Do you often find that you have made up your mind too late?	−0·011	0·475	−3	12

Appendix Table (*cont.*)

		Loading			
		E	N	D_E	D_N
96.	Do you like to mix socially with people?	0·492	−0·170	23	−8
97.	Have you often lost sleep over your worries?	−0·050	0·509	−6	16
98.	Are you inclined to limit your acquaintances to a select few?	−0·504	0·171	−21	12
99.	Are you often troubled about feelings of guilt?	0·068	0·543	−12	20
100.	Do you ever take your work as if it were a matter of life or death?	−0·134	0·152	−24	11
101.	Are your feelings rather easily hurt?	−0·050	0·492	−3	16
102.	Do you like to have many social engagements?	0·543	0·002	29	0
103.	Would you rate yourself as a tense or 'highly strung' individual?	−0·068	0·452	−3	17
104.	Do you generally prefer to take the lead in group activities?	0·248	−0·326	16	−6
105.	Do you often experience periods of loneliness?	−0·031	0·460	−2	11
106.	Are you inclined to be shy in the presence of the opposite sex?	−0·427	0·263	−19	21
107.	Do you like to indulge in a reverie (daydreaming)?	−0·142	0·360	8	11
108.	Do you nearly always have a 'ready answer' for remarks directed at you?	0·332	−0·163	19	−6
109.	Do you spend much time in thinking over good times you have had in the past?	0·117	0·368	8	18
110.	Would you rate yourself as a 'happy-go-lucky' individual?	0·623	−0·126	30	−9
111.	Have you often felt listless and tired for no good reason?	0·007	0·584	−1	13
112.	Are you inclined to keep quiet when out in a social group?	−0·584	0·217	−28	23
113.	After a critical moment is over, do you usually think of something you should have done but failed to do?	0·097	0·476	−8	11
114.	Can you usually let yourself go and have a hilariously good time at a gay party?	0·536	−0·218	29	−14
115.	Do ideas run through your head so that you cannot sleep?	−0·165	0·344	−11	15
116.	Do you like work that requires considerable attention?	−0·223	−0·087	−3	−3
117.	Have you ever been bothered by having a useless thought come into your mind repeatedly?	−0·058	0·324	−21	29
118.	Are you inclined to take your work casually, that is as a matter of course?	0·208	0·096	10	0
119.	Are you touchy on various subjects?	0·126	0·368	−5	27
120.	Do other people regard you as a lively individual?	0·576	−0·077	18	−13
121.	Do you often feel disgruntled?	−0·088	0·559	−6	20
122.	Would you rate yourself as a talkative individual?	0·384	−0·035	30	−8
123.	Do you have periods of such great restlessness that you cannot sit long in a chair?	−0·035	0·419	0	19
124.	Do you like to play pranks upon others?	0·283	0·004	8	−1

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PREDICTING HYPNOTIZABILITY FROM THE MAUDSLEY PERSONALITY INVENTORY

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In an attempt to repeat the findings of Furneaux & Gibson, 142 undergraduate students at Stanford were hypnotized by the procedures of the Stanford Hypnotic Susceptibility Scale and a group adaptation of this scale. All subjects were also given the Maudsley Personality Inventory, including the Extraversion, Neuroticism, and Lie scales. When the subjects were subdivided into personality classifications according to the procedures of the Furneaux & Gibson study, significant differences between subgroups were found, but these were opposite to the significant differences found in the London study, 'liars' being below average in hypnotizability in England, and above in America, and, among the 'honest' subjects, Stable Extraverts and Neurotic Introverts being more hypnotizable than Neurotic Extraverts and Stable Introverts in England, less hypnotizable in America. Because of instability in successive samples in America, and some differences between English subsamples, it is suggested that elaborate hypotheses are not in order until there has been further replication both in England and America.

Correlations between scores on individual MPI scales and hypnotic susceptibility show a most unsatisfactory variation from sample to sample; hence the most acceptable conclusion must be that the MPI cannot yet be used as a predictor of susceptibility to hypnosis.

I. Introduction

Efforts to obtain correlations between scores on personality inventories and hypnotic susceptibility scores have been notably unsuccessful, the occasionally reported 'significant' correlations commonly disappearing upon replication (Hilgard & Lauer, 1962). Hence another reported success, that of Furneaux & Gibson (1961), using the Maudsley Personality Inventory, invites repetition. The present study reports such an attempted replication; unfortunately our results add one more case of failure to reproduce prior findings.

Furneaux & Gibson (1961) administered the Maudsley Personality Inventory (MPI) to a sample composed of 55 university students and 44 adult readers of a radical journal. We are indebted to Mr Furneaux for furnishing to us data for these two subgroups separately, and also an additional lie scale as used in the study. Although the lie scale had not yet been published, it seemed desirable, if we were to replicate the Furneaux & Gibson study, to use this scale along with the standard MPI scales. Each of these subjects, following a body-sway test, was hypnotized while lying on a couch by a technique involving suggested eye closure and repeated tests of eye catalepsy. The subject was classified as hypnotizable if eye catalepsy was successfully achieved within seven minutes, that is, if by that time the eyes were closed and the subject was unable to open them when challenged to do so. Under these circumstances 44 % of the subjects proved to be hypnotizable. Three personality scores were derived from replies to the MPI: a Neuroticism score (N), an Extraversion score (E), and a Lie score (L), based on replies to a new set of 18 questions not in the MPI manual (Eysenck, 1959). In their treatment of the data, Furneaux & Gibson first removed the 22 'liars' from the total; these were the subjects who scored 10 or more on the L-scale. The remaining subjects were then sorted into combinations of highs and lows on the other scales, yielding 27 Stable Extraverts (SE's), 15 Stable Introverts (SI's), 17 Neurotic Extraverts (NE's), and 18 Neurotic Introverts (NI's), following the general theory familiar in Maudsley data analysis (e.g. Eysenck, 1961). Thus the pool of 99 subjects was divided into five subgroups. The liars comprised one subgroup, and the scores of the remaining subjects were dichotomized at the sample means on the E and N scales to make four other subgroups. The results, stated most generally, showed the SE's and NI's to be more hypnotizable than the average, the SI's about average, and the L's and NE's least hypnotizable. We have constructed Table 1 from the data of Furneaux & Gibson, summarizing their data in chi square form. It will be seen that there is ample support for their conclusion that two of the groups are more susceptible than two other groups. On the basis of these statistically significant findings the decision was made to attempt a replication.

Table 1. *Hypnotizability in relation to a personality classification based on the MPI**

Personality Classification†	Hypnotizability‡ −	Hypnotizability‡ +	Total
SE and NI	16	29	45
NE and L	31	8	39
Totals	47	37	84

$\chi^2 = 16{\cdot}36$; D.F. $= 1$; $P < 0{\cdot}001$.

* Data from Furneaux & Gibson (1961) (Stanford analysis).

† Based on Extraversion, Neuroticism, and Lie-scales of the MPI

‡ Hypnotic susceptibility measured by eye catalepsy. Subjects unable to open eyes when challenged were considered hypnotizable.

II. METHOD

Subjects. Three separate samples of subjects were used. Fifty undergraduate male subjects were drawn from an introductory course in general psychology in the autumn quarter, 1961; 47 male subjects were drawn from an introductory course in the winter quarter, 1961–62; and 27 male and 18 female paid volunteer subjects were drawn from the winter 1961–62 undergraduate Stanford population. Subjects in the first two samples were required to participate in experiments as part of the course requirement while subjects in the third sample were volunteers. Although data indicate volunteers differ from non-volunteers in hypnotic susceptibility (Boucher & Hilgard, 1962), the results relevant to the MPI are similar for these groups. Data will be presented for the sample as a whole, $N = 142$, all of whom filled out the three MPI scales.

Hypnotic induction and the measure of hypnotic susceptibility. The 97 males of the first two samples were administered the Stanford Hypnotic Susceptibility Scale, Form A (Weitzenhoffer & Hilgard, 1959). This scale contains 12 suggestions scored pass or fail by the hypnotist, yielding a score continuum of 0–12. The 45 subjects of the third sample were administered the Harvard group adaptation of the Stanford scale (Shor & Orne, 1962). These subjects were hypnotized in small groups (six groups of 6 subjects, one each of 4 and 5 subjects); observers were present to score the responses. The use of the group scale is legitimate since data reported elsewhere (Bentler & Hilgard, 1962) indicate it gives results very similar to Form A of the Stanford scale. It is an adaptation of that scale, with only minor changes to make it amenable to group situations. It also yields a score continuum of 0–12. The staff of the Laboratory of Human Development did the individual hypnotizing. Although there are thus some minor differences in hypnotic procedures involved between the English and American studies, the data analysis calls merely for a separation of the more susceptible from the less susceptible. The overlap with Furneaux & Gibson's procedure is evident in that within the Stanford scale the eye closure item correlates $r_{bis} = 0{\cdot}57$ with the total scale minus that item and the eye catalepsy item correlates $r_{bis} = 0{\cdot}79$ with the total scale minus that item (Weitzenhoffer & Hilgard, 1959). The Furneaux & Gibson procedure, involving eye closure and eye catalepsy, must be tapping hypnotic performances very similar to those we have recorded. By dichotomizing the Stanford scores at a scale value of 8, so that those who score 0–7 are considered insusceptible and those who score 8–12 susceptible, the result is that 39 % of our sample are considered to be susceptible compared with 44 % of the Furneaux & Gibson sample. The proportions hypnotizable are thus very nearly equivalent, and the assumption that the criteria of hypnosis are similar is a reasonable one.

The distribution of scores on the MPI scales. Some method has to be adopted for cutting the scores on the N, E, and L scales of the MPI in order to make the Stanford and London procedures comparable. It is possible to use either *sample* means, having the advantage of producing more nearly equal divisions of cases for purposes of use in fourfold tables, or to cut at *population* means; the choice is arbitrary, and does not affect the theoretical interpretations according to the Maudsley practices. This is evident when two Maudsley reports have used the two methods. Furneaux & Gibson (1961) cut at the sample means; Furneaux (1961), using data from the same study, cut at the population means. In the Furneaux–Gibson analysis 35 cases among 99 were classified as 'neurotic' while in the Furneaux study 55 cases among the same 99 were called 'neurotic' (after removing 'liars' in both cases). Because the cutting scores are thus arbitrary, we felt that we could cut at the means of our sample without violating any of the assumptions of the Maudsley group. The actual cutting scores used in our sample were as follows, the scores indicated being the lowest score of the 'high' group: for L, 11; for E, 29; for N, 24. The L-scale was cut at a score of 10 in the Furneaux & Gibson study, yielding 22 % 'liars'. We cut our L-scale at 11, in order to have 22 % classified as 'liars'.

Table 2. *Chi-square of distribution of British and American students over personality classifications of the MPI*

Personality classification*	British sample		Stanford sample	
	N	%	*N*	%
Stable Extravert (SE)	27	27	39	27
Neurotic Introvert (NI)	18	18	28	20
Stable Introvert (SI)	15	15	19	13
Neurotic Extravert (NE)	17	17	25	18
Liar(L)†	22	22	31	22
Totals	99	99	142	100

$\chi^2 = 0{\cdot}21$; D.F. = 4; P = N.S. two-tail.

* Based on a mean split of Extraversion and Neuroticism scores on the MPI for each sample.

† British Liars scored 10+ points on the MPI L-scale; Stanford Liars scored 11+. (See text.)

It might be supposed that some socio-cultural differences between England and the United States and in the samples from these countries might produce differences in the responses to the MPI. That the internal relationships among the scales are quite consistent is shown by the percentage of subjects classified as SE, NI, SI, and NE, and L which results from cutting scores at the means of each sample. The results, compared with the Furneaux & Gibson results, are shown in Table 2. The distributions are very similar ($\chi^2 = 0{\cdot}21$, D.F. 4, N.S.).

Thus we appear to have our sample classified into two groups with about the same percentage of hypnotizability as the Furneaux & Gibson sample, and into five groups with the same proportions of personality syndromes as theirs. It is to be expected that if the relationship between personality test scores and hypnotizability is the same in our sample as in theirs, this ought to show up in the percentage hypnotizable within each of the five subgroups.

III. RESULTS

Hypnotizability and personality classification. Table 3 compares the hypnotic susceptibility of subjects over the personality classifications of the London and Stanford samples. In reading this table, percentage hypnotizable should be taken in reference to the total percentage hypnotizable in each group. It will be noted that in our sample, L's and NE's are most hypnotizable, and that the comparable London subjects are least hypnotizable.

The χ^2 analysis of our data is presented in Table 4. The results are exactly the opposite from the Furneaux & Gibson data presented in Table I. L's and NE's are significantly more hypnotizable than SE's and NI's.

It is apparent that our results not only fail to replicate those of Furneaux & Gibson, but are significantly in contradiction to theirs in the direction of relationship between personality measures and hypnotizability, although their scheme for classifying personality scores is used.

Table 3. *Hypnotic susceptibility of the London and Stanford samples*

Personality classification	% Hypnotizable	
	London sample*	Stanford sample†
Stable Extravert (SE)	70	41
Neurotic Introvert (NI)	55	18
Stable Introvert (SI)	47	21
Neurotic Extravert (NE)	18	56
Liar (L)	23	55
Total sample	44	39

* Hypnotic susceptibility measured by eye catalepsy, based on data from Furneaux & Gibson (1961).

† Hypnotic susceptibility measured by the Stanford scale. Subjects scoring 8+ points were considered hypnotizable.

Table 4. *Hypnotizability of Stanford students in relation to a personality classification based on the MPI*

Personality classification	Hypnotizability		Total
	−(0–7)	+(8–12)	
SE and NI	46	21	67
NE and L's	25	31	56
Totals	71	52	123

$\chi^2 = 7{\cdot}21$; D.F. $= 1$; $P < 0{\cdot}01$, two-tail.

Correlations between hypnotizability and single MPI scales. While Furneaux & Gibson prefer to combine scales as indicated in the previous section, it may be that our failure to replicate their results has something to do with the relationship of individual scales to hypnotizability. The result of combining scales is seldom to generate new correlations of any magnitude; when successful, the combination raises correlations that are already present with at least one of the scales. Because we have found in our studies that successive samples fluctuate widely in their correlations with personality measures, we have preferred to report results for separate studies. Hence, in what follows we shall report our successive samples separately, and also give separate data for the two fractions of the London study kindly provided by Mr Furneaux. A few data are also available from a study conducted at the University of Pittsburgh (Lang & Lazovik, 1962), and one at the University of Sydney (Thorn, 1961).

A number of correlations available are based on fourfold tables, and are expressed as tetrachoric correlations. Our first comparison is in these terms, comparing the

London and the Stanford results (Table 5). For Extraversion, the London study finds essentially zero correlations, while our Stanford results yield positive correlations throughout and are significant for all groups combined. For Neuroticism, the London correlation is significantly negative for students, and close to zero for the radical adults; our results are also highly variable, ending up close to zero for the total group. For liars, the London study shows negative correlations, while we show consistently positive ones, although only the combined results are statistically significant in both samples.

Table 5. *Tetrachoric correlations between single MPI scales and hypnotizability: London and Stanford results compared*

	Extraversion ('Liars' excluded)		Neuroticism ('Liars' excluded)		Lie scale	
	N	r_t	N	r_t	N	r_t
		London†				
Students	41	−0·09	41	−0·70**	55	−0·42
Radicals	36	0·00	36	−0·04	44	−0·54
Totals	77	−0·02	77	−0·38*	99	−0·46*
		Stanford‡				
Autumn 1961	38	0·49	38	0·22	50	0·23
Winter 1961–62	35	0·34	35	−0·34	47	0·47
Volunteers 1961–62	38	0·44	38	0·17	45	0·60
Totals	111	0·46**	111	0·01	142	0·29*

† Furneaux & Gibson (1961), and supplementary data.
‡ This study.
* Values thus marked yield significant χ^2 at $P = 0{\cdot}05$, two-tailed test.
** χ significant at $P = 0{\cdot}01$, two-tail.

Table 6. *Product-moment correlations between single MPI scales and hypnotizability: Various samples compared*

	Cases		Extraversion		Neuroticism		Lie scale.
	All subjects N	Liars excluded N	All subjects r	Liars excluded r	All subjects r	Liars excluded r	All subjects r
			Stanford†				
Autumn 1961	50	38	0·14	0·31	0·06	0·08	0·03
Winter 1961–62	47	35	0·29	0·13	−0·21	−0·05	0·25
Volunteers 1961–62	45	38	0·16	0·14	0·00	0·02	0·45**
Totals	142	111	0·20*	0·21*	0·00	0·16	0·16
			Pittsburgh‡				
Students	32		0·21		−0·24		—
			Sydney§				
Students	60		0·07		0·11		−0·06

† This study.
‡ Lang & Lazovik (1962), based on the Pittsburgh revision of the MPI (Bendig, 1959).
§ Thorn (1961).
* Correlations significant at $P = 0{\cdot}05$, two-tail.
** Correlations significant at $P = 0{\cdot}01$, two-tail.

Table 6 describes Pearsonian correlations, because these are available from the

Pittsburgh and Sydney studies. Our positive correlations with Extraversion, while low, are consistent with those of the Pittsburgh study, although the Sydney study shows an inappreciable correlation in agreement with the London findings. Excluding the liars does not benefit our correlations; for one sample, the correlation between Extraversion and hypnotizability rises when liars are excluded, but for the next it decreases, and the result for the total is no appreciable effect. The correlations with Neuroticism are quite unstable; one sample shows a negative correlation which would agree with the London and Pittsburgh findings, although not with Sydney. Again, excluding liars does nothing significant to the correlations except to insert more instability through reduction in sample size. Correlations with the Lie scale are all positive, though only once significantly so, against the negative correlation in London reported in the preceding table and the small negative correlation found in Sydney.

The conclusions from Tables 5 and 6 are not very encouraging. No single measure holds up over all samples; at the same time, each approaches significance in at least one sample.

IV. DISCUSSION

In order to explain the differences between the results that we obtained and those obtained by Furneaux & Gibson (1961), we need to examine very carefully the differences between our studies. Let us consider these in some detail, to determine, if we can, where to look for an explanation of the discrepancy.

The measure of hypnotic susceptibility. Because subjects are sorted in both studies by similar kinds of hypnotic procedures (induction through eye closure, and tests heavily weighted with challenges such as eye catalepsy), yielding similar percentages of hypnotizable subjects, the measures appear comparable. It is clear from personal discussion, however, that more authoritarian commands are used by the Maudsley workers than by the Stanford workers, whose approach is more permissive. Their subjects lay on a couch; ours sat upright. It is possible that such differences affect the distribution of susceptibility to hypnosis. If the unlike approaches produced resistance to hypnosis in different fractions of the population, this might be reflected in differing correlations of hypnotizability with personality measures. This conjecture is, of course, subject to empirical test.

The Maudsley Personality Inventory in England and America. It might be that cultural norms in England (favouring introversion among students), compared with those in America (favouring extraversion), might yield distributions of scores on the MPI which have to be differently interpreted because the same score may have unlike significance for social adjustment in the two places. The manual of the MPI (Eysenck, 1959) does in fact show American students to average higher than the English quota sample in extraversion (means, 28·53 against 24·91), while the differences in neuroticism are inappreciable (means, 20·91 against 19·89). Because the groups (SE, NE, SI, NI, L) that result when the subjects are subdivided by combined scores represent comparable proportions of the two samples (English and American) the suggestion is strong that despite some small differences the tests are interpreted very much alike in the two cultures. Hence it seems unlikely that cultural differences in response to the MPI provide the basis for the difference in results, although it is hard to rule out the possibility entirely.

Sampling problems. The samples in England and in the United States were drawn somewhat differently, but this should not have made very much difference in view of the manner of sorting subjects according to scores on the MPI. There is a sampling problem involved, however, in that the London results were not locally replicated. The fact that two London subsamples yielded generally similar results on the Lie scale is not crucial, in that the decision to eliminate the 'liars' was made after the total data were in. Perhaps on a new sample, drawn in the same way, the 'liars' would not show the same relationship to hypnotizability. There are, in fact, some differences between the two subsamples of the London study; had only the students been used, for example, the Stable Introverts (SI's) would have been classified as more hypnotizable (71 %) than the Neurotic Introverts (NI's) (50 %) reversing one general conclusion from the study. Perhaps before more elaborate hypotheses are developed to account for the disagreements between the London and Stanford studies one might wish for a replication of the London study on its home ground as well as abroad.

The single-scale correlations between the MPI and hypnotizability show a most unsatisfactory fluctuation from study to study, suggesting that such a type of measure is unlikely to serve very well in the prediction of susceptibility to hypnosis.

This study originated within the Laboratory of Human Development where the research on hypnosis is supported at present by grants from the National Institute of Mental Health (Grant M-3859) and the Robert C. Wheeler Foundation. The investigation was completed while one of us (P.M.B.) held a training fellowship under the U.S. Public Health Service.

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AN APPLICATION OF INTER-PERSON ANALYSIS IN PERSONALITY ASSESSMENT*

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A simplified form of factor analysis of persons was applied to test performance in a selection procedure associated with different patterns of two-part self-description (i.e. as one might be seen by a friend, and as by a critic). Self-descriptions containing irrelevant or trivial features were shown to carry, on the whole, predominantly unfavourable implications. In most cases, however, minority groups were characterized by fairly consistent favourable patterns of test performance. Classifications based on the distribution of praise and criticism were also shown, in some cases, to be non-homogeneous, and no single form of imbalance appeared to be uniformly unfavourable. In particular, role-reversal, as between the 'friend' and the 'critic', appeared to be a favourable rather than an unfavourable sign.

The use of inter-person analysis resolved certain anomalies which appeared in earlier studies by showing that these resulted from the mistaken assumption that a given self-description pattern had constant personality correlates.

INTRODUCTION

This paper is part of a series dealing with the use of projective techniques in a selection procedure. A paper (Semeonoff, 1959) read at a meeting of the Rorschach Forum and published in the *Rorschach Newsletter* dealt with the application of a factorial technique (Sandler's 'Delegate Analysis'—Sandler, 1958) to the description of personality patterns of candidates in the selection procedure in question, that of probationer counsellors for work with the Scottish Marriage Guidance Council. Further papers (Semeonoff, 1960, 1962) discussed the validity of two-part Self-description ('S-D') in this context, with particular reference to the implications of certain patterns of S-D response. Of these, the former paper (which was written and presented after the latter) dealt with an attempt to describe, in terms of test performance, the personality patterns of candidates who had produced self-descriptions of certain types. These candidates had all been given Rorschach as a supplement to the regular test procedure. The purpose of the present paper is partly to report the findings of a parallel study of candidates who had been given 'Object-Sorting' ('O-S') (see Rapaport, 1946), and partly to draw threads together and to make more readily accessible part at least of the contents of papers so far circulated only semi-privately in the *Rorschach Newsletter*.

An account of the selection procedure which has provided the data of this study has been given elsewhere (Semeonoff, 1958). Here it will suffice to say that candidates are assessed at a 2-day 'Selection Conference' by means of interviews, group activities and written psychological tests, both psychometric and projective. Selected candidates, mostly those who appear to be of border-line acceptability, are given individual tests, as mentioned above.

Additional comments on the methods used in handling the data for selection purposes will be found elsewhere (Semeonoff, 1962). In the selection context considerations of acceptability are always in the forefront, and one tends to make inferences from varieties of projective response on *a priori* grounds. The validity of some of these inferences has been demonstrated in the earlier papers in this series, but of greater general interest is the possibility, afforded by inter-person analysis, of recognizing 'types' of personality among candidates offering themselves for selection. As mentioned above, this was done in the first instance (Semeonoff, 1959) by the use of Sandler's rather elaborate 'Delegate analysis', and was based on a sample of 80 candidates—virtually all of those who, at the time of writing, had been given O-S.

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* Based on a paper (Semeonoff, 1961) read to the Scottish Branch of the British Psychological Society, 21 January 1961.

An application of Delegate analysis

Sandler's own description of Delegate analysis is reproduced verbatim as an Appendix to this paper. The following account of its main features, as applied in the present instance, is intended to be read independently of the Appendix.

The procedure is based ultimately on the correlation *of persons*; i.e. a measure is obtained of the similarity between pairs of persons in respect of a large number (or 'population') of 'test items'. In this case these were:

'Acceptance' or 'Rejection' ratings by the selection board and its members (6).
Individual questions in an intelligence test (16).
Varieties of Self-description response (5).
Single items and general features of O-S response (35).
Single items and general features of TAT response (33).
Miscellaneous (age, social class, etc.) (5).

The correlation measure used was a non-parametric one, namely that of 'joint frequencies', according to the formula $(\overline{a+d}-\overline{b+c})$, where a represents the number of test items in which both subjects showed what may be called a 'positive' response (usually a 'favourable' one); d the number of items in which both showed a 'negative' response; b and c the number of items in which one subject showed a positive and the other a negative response. In other words the measure is one of excess of similarity over dissimilarity: where it is positive the two candidates are more alike than unlike, the opposite being true where the measure is negative. If desired, the measure thus obtained can be divided by the total number of test items (here 100), giving a 'quotient' comparable to a correlation coefficient. This is not an orthodox statistic, but it is in fact very similar to one described by Clostermann (1958: review in *Brit. J. Psychol.* 1959, **50**, 286).

In Delegate analysis the total sample of persons is divided into subgroups or 'constituencies', usually on the basis of an *a priori* criterion, in this case degree of presumed acceptability. Each constituency is factor-analysed along the lines indicated, yielding a number of factors, each of which may be regarded as a composite individual showing different patterns of behaviour over the range of test items included in the analysis. These are the 'delegates', who represent the constituencies. The next step is to intercorrelate the delegates. In this way one arrives at second-order factors—or delegates—each of which represents a different type of candidate. The final step is to calculate factor loadings for each test item in each of the second-order factors. This amounts to determining the test performance of each of the second-order delegates, or to describing the characteristics of each of the types of candidate one has isolated.

Four such types, which emerged from the analysis, were as follows:

(1) A 'plain man' type, who might be said to take things at their face value, and who was less likely to find acceptance by the psychiatrist and psychologist on the board than by the 'lay' members.

(2) A candidate characterized by a high degree of 'personal involvement', who projects readily, and may find some features of the procedure somewhat disturbing.

(3) An unintellectual type, limited but stable, more likely to be accepted by the psychologist than by the lay selectors.

(4) An analytically minded intellectual type, successful in case-study discussion, but otherwise not showing any marked tendency to be rated as either acceptable or unacceptable. Very often this type of candidate appeared to be rather young for counselling.

These characterizations, described in fuller detail elsewhere (Semeonoff, 1959), are based on examination and interpretation of the test items showing the highest loadings in the relevant second-order factor. Some of the inferences drawn are admittedly conjectural, but in general they rest upon such distinctions as those between easy and difficult items in an intelligence test, or between relatively 'meaningful' O-S items—such as a pair of pliers or a pipe—and relatively 'meaningless' items such as a disc cut out of red paper, and on the commonly recognized implications of standard TAT pictures.

Self-description variables were shown to be associated with the first and fourth of the 'types' listed above, and therefore, presumably, with the other test items characterizing these types. A further study (Semeonoff, 1962) was then undertaken in order to throw more light on the relationship between self-description and the other test procedures employed. The method was, briefly, to use ordinary statistical tests (t and χ^2, as appropriate) to discover which TAT, O-S and Rorschach variables, from a limited range studied, discriminated between S-D's conforming to certain patterns and all other S-D's.

PATTERNS OF SELF-DESCRIPTION

Seven patterns, or modes of response in Self-description, were studied as follows:

'High E', a relatively long enemy's description.

'Low E', a relatively short enemy's description.

'F –', the 'critical Friend'.

'E +', the 'inverted Critic', i.e. praise present or implied in the enemy's description.

'A', mention of personal appearance.

'T', mention of trivialities, e.g. hobbies, and minor personal failings such as unpunctuality etc.

'I', descriptions couched in the first person.

It was found that all seven modes of S-D response were significantly associated with TAT variables; the three last-named, or 'qualitative' modes, with O-S variables; and all except 'E +' and 'A' with Rorschach variables. Conclusions with regard to the 'favourable' or 'unfavourable' nature of the S-D modes of response have been discussed elsewhere (Semeonoff, 1962), and are summarized in Table 1, along with comparable findings from the later work which forms the main subject of this paper.

INTER-PERSON ANALYSIS

The investigation just quoted was based on rather small numbers, and was to some extent shaped by statistical rather than psychological considerations. When one applies statistical tests such as those mentioned above, one states, in effect, that a certain criterion 'discriminates' when all or most members of a specified group are characterized by the possession of a certain quality, while people outside that group show a relative absence of that quality. To take an example: in order to be able to say that people who use the 1st person in self-description also tend to produce Egocentric content in TAT, it must also be true that people who do not use the 1st person tend not to produce Egocentric TAT's. This is a valid statistical principle but it does not necessarily apply to personality description in terms of projective categories. Different clinical groups often have certain patterns of response in common: witness, for example, the well-known overlap between 'neurotic' and 'organic' signs in Rorschach.

Table 1. *General implications of S-D patterns*

	Delegate analysis	Semeonoff (1962)	Inter-person analysis	
			Group R	Group O
'High E'	[Fav.*]	?	?	Fav
'Low E'	?	(Fav.)	? Fav.	?
'F−'	—	Fav.	A Fav. B Unf.	A Fav. B Unf.
'E+'	[?†]	Unf.	A? B (Unf.)	Fav.
'F−/E+'	—	—	—	Fav.
'A'	—	Unf.	Unf.	Unf.
'T' T_F T_E	Unf. Unf.	Unf.	A Unf. (T_F) ....‡ B Fav. (T_E)	T_F Unf. T_E A Fav. B Unf. T_{FE} Unf.
'I'	—	Unf.	A Unf. B Fav.	B Unf. A (Fav.)

Notes. Fav., Favourable; Unf., unfavourable; ( ) slight trend in direction shown; ?, conflicting evidence; A, larger subgroup; B, smaller subgroup.

* 'Low F'.

† 'Overpraise'.

‡ Not analysed separately but majority in Friend's or Enemy's description as shown.

It was felt, therefore, that a better idea of the characteristics of people producing the various types of S-D could be obtained by applying a form of inter-person analysis, based, in general, on the Delegate analysis described above.

Subjects

The investigation was planned in two stages. The first dealt with 42 candidates who had done Rorschach (Group R), and has already been described in the *Rorschach Newsletter* (Semeonoff, 1960); the second with candidates who had done Object-sorting (Group O).

With Group R an inter-person analysis was carried out for each of the S-D patterns using, except for F −, all or nearly all the candidates whose self-descriptions conformed to that pattern. Since many of the self-descriptions fell into more than one category, the groups overlapped to a considerable extent. The very large F − group was reduced to 16 (i.e. about the maximum size of the other groups) by excluding all candidates whose S-D's fell into more than one other category.

Since in Group O numbers were much larger, selection of cases became necessary in order to produce numbers comparable with those in the analyses for Group R. This was done by taking either extreme cases, or those which belonged to only a given category, or to only one other. 'Trivialities' was handled by making up separate groups according to whether the mention of trivialities occurred in the Friend's description (T_F), or in the Enemy's (T_E), or in both (T_{FE}). This was a partial reversion to what had been done in the Delegate analysis, where trivialities in the Friend's and Enemy's descriptions had been treated as separate variables. Finally, a new category was created by considering separately those candidates whose S-D were classifiable as both F − and E +; these were excluded from the F − and E + categories. A complete reversal of roles along these lines is described by Harris (1949, p. 66) as 'a strong pointer to instability'.

Table 2. *Constitution of groups analysed*

	Group R				Group O		
S-D pattern	No. analysed	Discards ('atypical')	No. in subgroups		No. analysed	Discards ('atypical')	No. in subgroups
'High E'	6	0	—		16	2	—
'Low E'	8	2	—		11	0	—
F−	16	0	(8, 8)		16	0	(11, 5)
E+	15	1	(7, 7)		14	0	—
F−/E+	—	—	—		14	2	—
A	8	1	—		18	0	—
I	10	0	(6, 4)		17	1	(11, 5)
T	13	0	(7, 6)	T_F	17	0	—
				T_E	10	0	(6, 4)
				T_{FE}	14	0	—

Details of the constitution of the subgroups on which the analyses were based are contained in Table 2.

Test variables

The test items were chosen in much the same way as for the Delegate analysis (see above) but were limited to 50 instead of 100. For both groups these included the six disposal ratings, the total score in a 20-item abstractions test and the four broadly based TAT variables used in the second study in the series (Semeonoff, 1962). The remaining 39 variables were all from Rorschach or all from O-S. The Rorschach variables are summarized elsewhere (Semeonoff, 1960); they included most of those in common use, together with four based on the present writer's modifications of accepted categories and three derived from Shatin (1955). The O-S variables were practically the same as those used in the Delegate analysis, i.e. adequacy of sorting and verbalization in individual items, plus incidence of various types of 'pathognomic' response in either part of the test.

RESULTS

In contrast to the Delegate analysis, the writer's intention in the present analyses was to extract a single factor in each case, the expectation being that each S-D pattern would yield a general factor which would epitomize the projective and other test performance most typical of the variety of S-D in question.

'Homogeneous' and 'bipolar' patterns

In the event this expectation was seldom realized. In an earlier paper (Semeonoff, 1962) the writer has suggested that on *a priori* grounds, at least, more than one rationale might be adduced for the interpretation of certain of the S-D patterns. In conventional factor analysis such a possibility might be confirmed by the emergence of several factors corresponding to different personality types. In the present analyses the evidence was even more striking, in that most of the samples either contained one or two candidates who showed predominantly negative 'correlations' with the other majority, or split themselves into two more or less equal subgroups showing positive correlations within the subgroup, and negative correlations with the subjects in the other subgroup. In other words, in these latter cases (hereinafter referred to as 'bipolar') positive and negative correlations may be said to have appeared in the manner commonly recognized as indicating the operation of a bipolar factor. One may interpret this as meaning that each of these modes of S-D response comprised productions by two contrasted personality types. Whether more than two might be involved cannot be determined without the extraction of additional factors.

In the cases where 'atypical' candidates appeared, it may be surmised that larger numbers might have helped to establish the existence of a contrasted 'type' associated with that particular S-D pattern. In the absence of such evidence, however, one can only recognize the occurrence of such atypical cases, and leave their consideration over for the time being.

Which S-D patterns emerged as homogeneous and which as bipolar can be seen from Table 1. It will be noted that there is good correspondence between the results for Group R and Group O: 'High E', 'Low E', 'A' are homogeneous in both cases; 'F −' and 'I' bipolar in both cases; only 'E +' is inconsistent. (T was treated differently in the two groups, and on the whole the tendency is for this category to appear as bipolar.) Very close agreement could not have been expected, since, as already stressed, the candidates included in the analyses were mostly 'border-line', and, in addition, allocation to the Group R or the Group O itself presupposed differences in personality: candidates who were given Rorschach usually either were more sophisticated or had produced test behaviour suggestive of more serious personal problems of their own. Thus, while the 'I' category in each group produed a small 'favourable' subcategory (see Table 1 and below), the characteristics common to either subcategory were rather different.

Favourable and Unfavourable indications

As will be seen, Table 1 sums up the findings in terms of the categories *Favourable* and *Unfavourable*, by which is to be understood a judgement based on a combination of likelihood to be regarded as acceptable or unacceptable for work in counselling, and indications of stability, maturity and insight in the field of personal relationships. The categories may be thought of as being applied in much the same way as they are used—perhaps loosely—by many clinicians in relation to projective response in general.

The earlier report of the analysis of the Group R (Semeonoff, 1960) contains an account of the Rorschach and other variables common to *all* candidates falling into a particular S-D category. Interpretation in terms of Rorschach rationale was attempted, and appeared to find confirmation from other sources. In the case of Group O, the nature of the O-S variables made such interpretation difficult, although in individual cases differences in ability to conceptualize in relation to different types of object seemed to be of relevance. The separate varieties of 'pathological sorting', again (see Semeonoff, 1962), occurred too infrequently to be amenable to statistical treatment.

Table 3. *O-S performance in S-D 'patterns'*

		Mean no. of Inadequate responses				
		Part I (sorting)	Part I (verb)	Part II (verb)	Mean no. of 'Pathological' sortings	No. of cases I < II
High E	(14)	2·9	3·1	1·6	1·4	0
Low E	(11)	2·5	2·4	2·0	1·5	1
'A'	(18)	3·7	4·3	4·1	3·1	3
'I' A	(11)	3·5	3·5	2·5	1·2	0
B	(5)	4·8	5·6	7·0	5·0	3
T_F	(17)	2·5	3·2	4·5	1·9	4
T_E A	(5)	1·8	2·0	1·2	2·0	0
B	(4)	4·5	5·2	9·0	5·5	2
T_{FE}	(14)	3·3	3·4	3·5	2·0	2
F− A	(11)	2·7	3·2	1·3	1·6	0
B	(5)	5·2	6·0	10·4	4·2	2
E+	(14)	3·4	3·1	2·0	2·5	3
F−/E+	(12)	3·2	3·1	2·3	1·8	0
All	(89)	3·2	3·3	3·1	2·2	13

Last column: Score in Part I lower than score in Part II; recognized as an unfavourable indication.

Table 3 compares certain features of all-over O-S performance for the different S-D categories. No attempt has been made to calculate significances, for the reason quoted above (p. 74). The reader is invited to draw his own inferences, which can, of course, be only of a very general kind.

The following comments summarize the contents of Table 1, which, as already mentioned, deals with all the investigations covered in the series.

Physical Appearance is clearly unfavourable.

First Person and *Trivialities* are on balance unfavourable, but in each case there is a relatively small subgroup in which that pattern of self-description is associated with favourable personality indications. It would appear, further, that trivialities in the Enemy's description are a more unfavourable sign than when they occur in the Friend's. A suggested interpretation of this is that mention of minor weaknesses, etc., by the 'Enemy' indicates a feeling of guilt, where the same in the 'Friend's' description suggests tolerance of such failings inasmuch as they are indeed 'minor' in character.

In contrast to the two types just mentioned, 'F − ' is predominantly favourable, with a small deviant group for whom the indications are unfavourable. The anomaly previously noted (Semeonoff, 1962), namely that favourable projective indications were here associated with tendency to rejection, is partly resolved when one noted that in both Group R and Group O the accepted candidates nearly all fall into the 'Favourable' and the rejected into the 'Unfavourable' subcategory.

The 'favourableness' of 'F − ' is borne out by the result for the SD's containing both F − and E +. This would suggest that this pattern may indicate not confusion of roles, as Harris (1949) supposes, but the sort of critical awareness that would seem to be a *prima facie* qualification for counselling.

For 'E + ' by itself the evidence, such as it is, appears to be about evenly divided. It mây be relevant to stress here that the work so far reported has been concerned with mostly 'border-line' candidates, i.e. those whom the psychologist had selected as requiring more detailed study, and it may well be that a scrutiny of the S-D's of the remaining candidates could throw some light on this question.

The other two S-D variables are purely quantitative, and it is not surprising that short or long S-D's seem to have different implications in different people, and that desirable personal qualities and acceptability do not necessarily go hand in hand. It may be noted, however, that for Group O, unlike the other groups studied, imbalance in either direction between the two descriptions seems to be an unfavourable sign. Once again the 'border-line' nature of the candidates studied may be relevant, together with the fact that on the whole O-S, as against Rorschach, is usually chosen for candidates of probably less complex personality characteristics.

It is difficult to summarize the position, but the following observations are offered:

(1) *'High E'.* In Group R the projective indications are on the whole more favourable than the selectors' recommendations; in Group O there is a tendency towards acceptance, the O-S performance is relatively undisturbed (see Table 3), and the TAT indications are favourable. This type of S-D response was not treated as such in the Delegate analysis study (Semeonoff, 1959), but 'Low F', which would satisfy a very similar criterion, was shown to be characteristic of a type of candidate who was highly acceptable but performed poorly on O-S. A possible interpretation of this pattern is that of a fundamentally stable personality experiencing difficulties in adjustment, which may perhaps not extend beyond the test-situation.

(2) *'Low E'.* In Group R the converse of the position for 'High E', stated above, holds. In Group O the O-S performance itself is good, and the level of acceptability high, but the TAT indications much less favourable.

In both cases, therefore, there is a slight balance of evidence on the side of 'favourable'. In a 'good' candidate this may suggest that a short critic's description has been the result of criticism having already been appropriately incorporated in the friend's description, while a long one may stem—rather similarly—from a genuine recognition of one's failings. In view of all we have said it is perhaps surprising that in neither group did either the 'High E' or the 'Low E' categories prove to be 'bipolar'.

A final incidental point worth noting is that the intelligence score showed no significance anywhere in either series of analyses.

Appendix

SOME NOTES ON 'DELEGATE ANALYSIS'

BY JOSEPH SANDLER*

Delegate analysis is a technique which was evolved in 1956 at the Tavistock Clinic, London, in order to simplify factor-analytic procedures which might otherwise involve a prohibitive amount of labour. Through the use of delegate analysis the scores of a large number of persons (N) on a large number of tests (n) can be factor-analysed. The method involves some loss of exactitude when compared with the full test factor analysis or the corresponding full person factor analysis, but the saving in time and effort more than compensates for this.

The essence of the procedure is to use a population of m 'delegates' instead of the full set of N persons. These 'delegates' are 'elected', through a procedure which will be described later, and represent (factorially) the original N cases. They are not selected individual persons, but rather hypothetical composite persons. The number of 'delegates' used will depend on the particular research, but it is present practice to use 15 'delegates' to represent 100 persons.

The method which is described here has been devised for the analysis of scores which are dichotomous in form (e.g. responses to the Tavistock Self-assessment Inventory, which can be categorized as either 'true' or 'false').

Metric. A score of -1 is allotted to an answer of 'false' and $+1$ to an answer of 'true'. The choice of this metric (rather than a score of 0 for 'false' and $+1$ for 'true' or a score corrected for the standard deviation of each item) was based upon the requirement that each person be given *equal* weight initially in the factor analyses. The use of the $-1:+1$ metric ensures that the sum of the squares of the scores of any one person is always equal to n, the number of tests. It also has the advantage that the product (q) of the scores in the n tests of any two persons, is constant irrespective of any change in the direction of scoring of any one or more tests. This is an important requirement in work with personality inventories, where the direction in which an item is to be scored is often decided upon arbitrarily.

The 'constituencies'

If 100 cases are to be used, it is convenient to divide the population of 100 into 5 'constituencies' of 20 persons each. The allocation of persons into 'constituencies' can be made at random, but the following method has been found useful. It has the advantage that the effect of the first general (centroid) factor, which is usually considerable with data of the sort dealt with by this technique, is minimized in the constituencies (but not in the later analysis). It is the factors other than the first with which we are usually most concerned in problems of psychological classification.

The n items are arranged so that each item has a positive correlation with the sum of all the items. A positive score on any item will then be one in the direction of a positive contribution to the total score. This step is usually not at all difficult. The total score of any one person on the n items is then a representation of his score on the first general centroid factor of the items (Burt, 1940).

The 100 persons are then ranked in order of their total scores on the n items (now scored as described above). The top 20 are allocated to group A, the following 20 to group B, etc., the final 20 being placed in group E. (The number of cases in each group and the number of groups is a matter of convenience.) Each group now contains a set of 20 persons who resemble one another in respect of their 'general factor' scores. The result of this selection is to minimize though by no means to exclude the influence of the first general factor if any one of the sets of scores of the 20 persons in the n tests is subjected to factor analysis.

The selection of 'delegates'

The 5 sets of persons are then subjected to factor analysis. (Thurstone's Centroid method is usually satisfactory.)

For each group, if we consider the scores of the 20 persons in the n tests to be written in an $n\times 20$ score matrix S, then we can compute the 20×20 matrix $Q = S'S$. The factor analysis of

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Q will yield a set of k common factors, each of which represents a hypothetical composite person (Sandler, 1949).

Each delegate from the group of 20 persons corresponds to one of these factors, and we obtain an estimate of the scores of the delegate in the n tests as follows.

For each test, the scores of the 5 persons with the highest positive saturations are summed. From this the scores of the 5 persons with the highest negative saturations are subtracted. In effect, the scores of the 10 persons concerned are added, the scores of the first 5 (those with positive saturations) being weighted $+1$, while the opposite 5 persons (those with negative saturations) have their scores multiplied by -1. Finally each sum is divided by 10 to obtain an average score, or the 'delegate' score.

It should be noted that the number of common factors might vary from one group of persons to another, but in practice it has been found valuable to extract the same number of factors from each group of persons.

The method of obtaining the delegate scores outlined above is appropriate for those factors after the first. If the first factor is to be included, the procedure would be to take the 10 highest saturations irrespective of sign (which in most cases will all be positive) and to take the mean of these 10 persons; if, however, one or more of these persons has a negative saturation, then he should be reflected as for the bipolar factors.

The factor analysis of the delegates

If the scores of the m delegates to be used are written into an $n \times m$ matrix Z, we can proceed with a person factor analysis as before.

If we assume that the scores of these m delegates can be expressed in terms of h orthogonal factors, we can write $Z = FP$, where F is a factor matrix of order $n \times h$, and P is a population matrix of order $h \times k$.

Consider for the moment the principal component solution F_p. Then $Z = F_p P_p$.

We have $F'_p F_p = V$, a diagonal matrix. $PP' = I$, an identity matrix.

Then Q, the $m \times m$ matrix of delegate product-sums is given by

$$\begin{aligned} Q &= Z'Z, \\ &= (F_p P_p)' (F_p P_p), \\ &= P'_p F'_p F_p P_p, \\ &= P'_p V P_p, \\ &= P'_p V^{\frac{1}{2}} V^{\frac{1}{2}} P_p. \end{aligned}$$

Let $V^{\frac{1}{2}} P_p = H_p$. Then it is evident that Q may be factored into a person factor matrix H_p, and it is clear that H_p is the principal component solution, for

$$H_p H'_p = (V^{\frac{1}{2}} P_p)(V^{\frac{1}{2}} P_p)' = V^{\frac{1}{2}} P_p P'_p V^{\frac{1}{2}} = V.$$

The matrix F_p can be obtained as follows: $F_p = F_p P_p P'_p = Z P'_p$. But $V^{\frac{1}{2}} P_p = H_p$. Then $P_p = V^{-\frac{1}{2}} H_p$, and $F_p = Z H'_p V^{-\frac{1}{2}}$. In practice the Centroid solution H_c is used instead of H_p and yields satisfactory results, particularly if rotated to a principal component solution (Thurstone, 1947).

In order to compare the factor saturations in F with one another, it is worthwhile taking the step of dividing each value in F by w_j (where j is the test concerned, and $w_j = \left(\sum_{1}^{k} z_{ji}\right)^{-\frac{1}{2}}$). z_{ji} is the score of the ith delegate in the jth test.

The factor saturations in the new factor matrix then range between -1 and $+1$.

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PUBLICATIONS RECENTLY RECEIVED

Uncertainty and Structure as Psychological Concepts. By W. R. Garner. New York and London: Wiley. 1962. Pp. x+369. 68*s*.

On first glancing at this book and on leafing perfunctorily through it, one might well come to the conclusion that this is just one more elementary introduction to information theory for psychologists with summaries of experiments in which information-theory concepts have been used, although it looks rather larger than most of the books and articles in that category have been. As one reads the book, however, one finds that there is much more to it than that.

It has, in fact, several refreshing characteristics that are not encountered over-frequently in psychological literature. There is, for example, the author's modest and restrained tone. He is not afraid to put his points strongly, when he feels that they should be brought to the reader's attention forcibly. But he refrains from anything resembling a 'pushing' or 'selling' style. He evidently feels, and he wishes the reader to feel, that revolutionary developments may be just around the corner, as far as psychology is concerned, thanks to the impact of the concepts of information theory. But he does not say so definitely. He is content to do his bit towards bringing things nearer to the corner and to let the future show us what is in store when the turn has been completed. Nevertheless, he cites a good deal of research to show that the concepts that he is discussing have already made quite a difference and prompted important findings that would otherwise have been missed. His avowed aim is not to put forward a theory in the true sense of the word. In fact, he refrains from using the term 'information theory', in case it might lead to such a misunderstanding. He merely offers a conceptual framework into which a variety of psychological phenomena can be fitted and suggests that such a framework may perform a valuable function in raising new questions.

In the first chapters, the author provides a clear, methodical introduction to the concepts of information theory—he prefers not to refer to them in this way, but the fact remains that they have arisen out of information theory. His introduction is skilful and clear, but probably no more so than several others that have been provided by other writers. He considers first 'univariate uncertainty', or uncertainty as a property of one set of events, and then goes on to 'bivariate uncertainty', which covers what is usually discussed under the heading of 'information transmission' or 'communication', although, as the author rightly points out, it is more general than those terms suggest. At this point, he goes on to the concepts of 'multivariate uncertainty', developed by himself and McGill and applicable to situations in which events are reflecting information from several sources, whose information contents may be related in various ways. This analysis gives rise to statistical techniques which are, in many ways, analogous and similar in function to those of analysis of variance, although they are of much wider applicability.

The latter half of the book contains the most original material. The author presents a conception of 'structure' and a way of quantifying it on the basis of extensions of multivariate uncertainty analysis. Structure is treated as a matter of redundancy and constraint. A set of patterns possesses redundancy, which implies constraint, whenever the possible combinations of values of variables do not appear with equal frequency. The author actually concentrates his discussion on cases where some of the possible combinations do not occur at all but the others are all equally likely. He distinguishes between internal constraint, which means correlations among the variables making up the patterns, and external constraint, which means correlation between patterns of what he calls 'predictor' variables (corresponding to what have more often been called input signals) and a 'criterion' variable (corresponding to what has usually been called the output signal). Among the principal points that he stresses are (1) that external and internal constraint are interchangeable, in the sense that, given a certain amount of total redundancy, the more one has of one the less one has of the other; (2) that 'uncertainty is a prerequisite to structure', since structure, which includes information transmission and internal correlation, cannot exist without variety; and (3) whether increases in redundancy help or hinder discrimination depends on the *form* of the redundancy and on such factors as whether the subject has enough time to sample all the properties of the stimulus pattern.

The plan in most chapters is to expound the mathematical notions first and then to review experiments showing how these notions are related to psychological phenomena. Most of the mathematical discussion is lucid and accessible to the non-specialist who has a little probability theory and is willing to put in a reasonable effort. There are some passages in the latter half of the book in which, although the reader is given all the information that he needs to follow the argument, his span of apprehension may be strained a little if his mathematical thinking is out of condition. At just a few places, the author leaves out some of the steps in his reasoning, so that the average reader will have to take what he says on trust.

During the brief history of information theory, its reputation among psychologists has undergone many vicissitudes. There have been those who hoped to derive fully fledged theories of behaviour from it, there have been those who have aimed to cut it down to size as one set of statistical techniques among all the others in our armory, and there have been those who have gloated over its ignominious demise as a fad that has had its day without leaving much of a permanent mark. The striking maturity of attitude in Garner's book is characteristic of a stage at which a just appraisal can be attempted. It puts one in a position to see the contribution of information-theoretic concepts in perspective and to speculate a little about their psychological future. The literature reviewed in the book can certainly silence anybody who claims that information theory has made little difference to psychology, having merely provided a jargon for expressing things that could just as well be expressed in other languages. Experiment after experiment cited by Garner corroborates what Broadbent has maintained perhaps more insistently than anybody else, namely that the effects of a stimulus pattern on behaviour depend on what other stimulus patterns might have occurred instead. It is hard to suppose that this important fact could have been discovered if information-theoretic concepts had not driven research in the right direction.

What emerges from Garner's book is the recognition that information theory is something that any psychologist, no matter what his speciality, ought to master and to apply to his subject matter. If every psychology student became acquainted with it quite early in his career, it could be an aid to clear thinking from the start. At the same time, these concepts, as Garner emphasizes, can only raise problems. They can help us to describe phenomena as a first step, and only a first step, to explaining them. It is by no means impossible that the concept of uncertainty will have the propelling and integrating effect on psychology that the concept of energy had on the physical sciences in the nineteenth century, although it is certainly too early to prophesy that it will.

Since the author sets himself such reasonable but circumscribed aims and achieves them so commendably, it almost seems out of place to voice some minor discontents. It might, however, be suggested that the author was wrong to hope that all the problems of structure would yield to analysis of constraint. Garner is thinking of 'structure' as something that stimulus patterns have more or less of. There is, however, a different concept of structure as something of which things have different kinds. This is the one that appears in logic and mathematics, where it is defined in terms of isomorphism, etc. In fact, mathematics has sometimes been defined as the 'science of structure', in this sense. It is also a sense that concerns psychologists very seriously, especially when they come to consider perception, thinking, and brain functioning. Garner might have made clear the distinction between the *amount* of structure in a pattern and the *nature* of the structure of a pattern, and he might have discussed the relations between them. Secondly, the experiments that he cites pertain almost entirely to perception, discrimination, remembering and motor skills. There is no mention of thinking or of motivation. Yet these are areas that information-theoretic concepts may do a great deal to revitalize, as several current developments are hinting.

D. E. BERLYNE

An Inquiry into the Freedom of Decision. By HARALD OFSTAD. Oslo: Norwegian Universities Press; London: Allen & Unwin. 1961. Pp. xvi+391. 42*s*.

Ofstad, a Norwegian philosopher of some distinction, is at once at home in painstaking analyses of ethical statements, and also—this is comparatively rare among modern philosophers—he is psychologically well informed. Thus, though interested in philosophical analyses he also takes into account relevant psychological and scientific issues. In this book he is largely concerned with analysing what we mean when we say someone has freedom of decision. Any discussion about free will is, he thinks, unfruitful unless we first carry out such an analysis. A large part of this work therefore consists in distinguishing the different meanings possessed by the expression

'(The person) P decided freely in (the situation) S'. Six main kinds of such analyses are distinguished, and these in all cover 161 different meanings. Though this expression can be given all the meanings discovered by Ofstad, some of them are, nevertheless, far removed from what the ordinary person has in mind, when he says he has free will.

Central to this kind of clarification of the free will issue, and indeed a reason for it, is the distinction drawn by Ofstad, and for that matter most other philosophers, between normative and factual statements. To prove or disprove that we have freedom of decision implies, he argues, that we can prove or disprove a certain normative statement, which depends for its truth or validity on certain criteria or conditions of duty or ethical blame.

A psychologist might object to the sharp distinction drawn between normative and factual statements, especially as factual statements are themselves never free of some interpretation. Although it may be true that discussions about free will are relative to an ethical system, are these systems only dependent for their truth on certain rational criteria? Ethical statements have to be anchored to our ethical beliefs, and these beliefs are psychological phenomena which can certainly be factually studied. There may be a level of abstraction in which we can examine the logic of ethical statements without any such reference. But this is to assume that ethics is a formal discipline like mathematics. Such a position is not one which would win much favour among psychologists. They would argue that psychological data are relevant to the way we make ethical judgements, and also to the criteria in terms of which we evaluate the truth and falsity of statements expressing such judgements.

W. MAYS

Social Psychology through Experiment. Edited by GEORGE HUMPHREY and MICHAEL ARGYLE. London: Methuen. 1962. Pp. viii + 208. 21*s*.

The starting point for this latest Methuen Manual is the idea that 'scientific subjects cannot be taught properly unless the student can have first-hand experience of the phenomena being described', and indeed that 'experiment is a pre-eminently searching method of education' in its own right. For years this has been acknowledged and practised in the spheres of cognitive and perceptual psychology. Now for the first time we are presented formally with a series of topics in social psychology, to be investigated in the conventional, two-hour, afternoon practical period.

Hence the title should perhaps have been the more modest 'Teaching Social Psychology through Experiment', and in so far as this is a handbook of concrete experiments, the emphases in the two Introductions are surely a bit misplaced. The book has minimal general literary interest, and Professor Humphrey will therefore be preaching to the converted on the validity and necessity of performing empirical investigations at all in this area of human behaviour. This may still be a controversial view in an Oxford common room, not so for the demonstrator in a Psychology Department. However, as I have indicated, some good points are made about the value of actually performing an experiment: 'The time will come when those responsible for the teaching of the succeeding generation will regard learning to experiment as being equally important with learning to read and write'. This basic point, being reinforced by the fact that the transition from 'book-knowledge' to 'personal knowledge', is a vital one in the whole educational process. The social field, where independent variables are particularly numerous and elusive, provides perhaps the ideal challenge.

The book proper consists of seven chapters all by psychologists teaching in British universities (viz. Michael Argyle, Gustav Jahoda, J. F. Morris, A. N. Oppenheim, D. R. Price-Williams, Peter Robinson, Henri Tajfel, and Thelma Veness), focused either on particular problems, e.g. motivation and conflict, social class, or on special skills and techniques, e.g. interviews, small-scale social surveys. The pattern of the chapters is in the form of a short natural history of the topic with reference to sources, the actual procedure to be followed, the statistical analysis (usually non-parametric of course), and the conclusions that might be drawn. Each contributor presents three concrete experiments, with suggestions for their variation. They are suitable for a range of student sophistication and need not be confined to Honours courses—some could be carried out with first-year students or adult evening classes. The experiments are usually versions of a 'classic' and usually have the personal endorsement of the author that they 'work' with British students. The latter point is sensibly noted to be vital for continuing morale.

Treating this then as a form of cook-book—how successful is it? Obviously even before its advent, some social material had been included in experimental classes. On the question of novelty, the small group material and the serial reproduction paradigm of rumour are least interesting. The interview and survey chapters on the other hand will probably provide new points of departure. It is good to see that they have been especially well devised for the specific purpose at hand.

Considered from the view of applicability, a few topics seem to have slipped in from a volume of readings in general experimental psychology. Although Lambert *et al.* devised a good experiment involving the measurement of French-Canadian stereotypes, one can think of few places, apart from certain areas of bilingual Wales, where the subtlety and point of the original would not be completely lost. In the same chapter the study of colour recognition has very little social relevance, as has the one on the inference of traits, which is essentially a problem in semantics. However, the allied discussion of social perception and its relacement by the term 'social cognition' is perhaps the most exciting in the book.

Down to earth practical advice about the details of procedure is one of the best things in the book. The authors are able to anticipate points of difficulty and give ample warning, whether it is about the finer points of an apparatus or a caution about 'interviewer's itch'. Social investigations suffer from the peculiar difficulty of their close relationship to 'real life', and the problem of feasibility from the view of public relations must not be forgotten. Most suggestions pass this test, although perhaps the problems consequent upon, if not inherent in, the consumption of alcohol for the purpose of inducing manifest sexual imagery will defeat the courage of many demonstrators.

It is suggested that one skill which may usefully be learned in a course such as this is the general handling of, and demeanour towards, subjects. Here all the contributors set an exemplary standard, not only in the narrow sense of providing a plausible task, but in the broader, humanistic frame. The subjects' morale and interest are kept in mind, their intelligence is not underestimated, their rights are respected, and their self-esteem is upheld.

As it is seen, this is a book intended strictly for experimenters, be they advanced students or lecturers. Its market therefore would appear to be relatively small. Considered as a publishing venture—would it not have been more sensible to have had a less sumptuous and less permanent format, bringing it within the reach of every member of the class and making it more amenable to revision? Perhaps an electro-type paperback manual on the US model might have been a better answer.

HALLA BELOFF

Society: Problems and Methods of Study. Edited by A. T. WELFORD, MICHAEL ARGYLE, D. V. GLASS and J. N. MORRIS. London: Routledge and Kegan Paul. 1962. Pp. vi + 586. 50*s*.

The title of this book is apt to be somewhat misleading. It is neither a text, nor a 'reader' consisting of previously published material, but a survey of various aspects of empirical social research in Britain today. The contributions are written by distinguished workers in different fields, and the high expectations aroused by their names on the cover are not disappointed.

The first part deals mainly with methods, covering such topics as sample surveys (Grebenik and Moser), mental measurement (P. E. Vernon), small group studies (Michael Argyle), analysis of social institutions (Bryan Wilson) and operational research (Norman Bailey). Some of these chapters are greatly condensed and need careful reading, though the effect is well repaid.

The second and larger part is divided into four sections: industry, social pathology, population problems and aspects of sociology. A few examples will be given, slanted towards psychology and allied spheres. The chapters on 'The Sociology of Industry' (Tom Burns) and 'Accidents at Work' (A. B. Cherns) are both illuminating discussions of their respective problems. There is an account of 'Personnel Selection and Vocational Guidance' (Rodger and Cavanagh), and an incisive new look at criminology (Leslie Wilkins). A number of chapters are devoted to social medicine and psychiatry; in one of these the unfashionable thesis is argued that social factors have not been proved to be casually related to the functional psychoses (C. J. Wardle). Other contributions of special interest for psychologists include 'National Intelligence' (James Maxwell) and 'The Sociology of Education' (Jean Floud).

The emphasis on 'psychological interests' which guided the above selection perhaps runs the risk of being misinterpreted: while it is often possible to learn something, even in one's special field, from a concise survey expertly produced, this is not what one should look for mainly in this volume; its chief value for psychologists will be to bring them up to date about what is happening in neighbouring fields, and the extensive references appended to most chapters should prove most helpful.

The content of this book reflects the prevailing emphasis on empirical research and theoretical discussions are usually meshed in with these. It might have been desirable to introduce some more pure theory, or even disciplined speculation. Welford's ideas on the relationship between experimental psychology and social studies are a case in point: they are highly controversial, but stimulating. Relative emphasis is of course a matter of editorial discretion, but there ought to have been some explanation of the complete omission of anthropology from a volume on 'society'.

Most of those concerned with the study of society sometimes become depressed about what they feel to be the slow progress of their subject. When in need of a tonic, they can hardly do better than compare this work with its predecessor of 23 years ago. A great fund of new knowledge and techniques has been accumulated, which is probably for the first time adequately displayed. This is no mean achievement, and all those concerned with the production of this volume deserve our gratitude.

G. JAHODA

Society and Self. A Reader in Social Psychology. Edited by BARTLETT H. STOODLEY. New York: The Free Press of Glencoe. 1962. Pp. xx + 713. $7.50.

Most of the material in this book appeared originally in the *American Journal of Sociology* and the *American Sociological Review.* The central theme is the relationship of behaviour to social structure, the latter being somewhat vaguely defined as 'systems of proprieties'. With such a loose framework there was plenty of scope for the editor to follow his inclinations: he chose mainly recent work, empirical in nature, but with a sprinkling of theory. A brief and agreeably unobtrusive introduction precedes each of the major sections, and they close with useful hints on further reading.

A list of section headings (e.g. 'Aspects of Psychosocial Process') would not be very informative, hence some individual articles will be mentioned to indicate what the book has to offer. Seeman ('The Intellectual and the Language of Minorities') paints a depressing picture of the defensive mentality of what were in fact assistant professors at a mid-western university. W. J. Goode ('Illegitimacy in the Carribean Social Structure') makes skilful use of his findings for a discussion of the role of the father in different societies. Christensen ('Cultural Relativism and Premarital Sex Norms') compares Danish with two regional American norms in a sociological and demographic study. In contrast to this, an investigation of male sex aggression on a university campus strikes one as being almost akin to a certain kind of journalism, in spite of the statistical tables showing the frequency of petting above and below the waist. Campbell and Pettigrew ('Racial and Moral Crisis: The Role of Little Rock Ministers') provide a thoughtful and topical sketch of varying responses to a profound dilemma. Ben-David's ('Roles and Innovation in Medicine') is a masterly essay on the sociology of knowledge. A series of three articles on social and occupational aspirations report on valuable investigations in this sphere. There is also an interesting study by Martin and Westie ('The Tolerant Personality') which redresses the balance away from the usual emphasis on authoritarianism.

The above is necessarily arbitrary, and many excellent articles have had to be omitted. Any 'Reader' of this kind may be viewed as a projective test, of the reviewer as well as of the editor. Taking the latter aspect, the present interpretation would be that the editor is a civilized sociologist with a wide range of interest. Social psychologists who (like the reviewer) lack the time and energy to keep fully abreast of sociological literature will welcome this book. It would probably be unwise to ask students to read it as a whole, but they could certainly profit from selective perusal.

G. JAHODA

Education and the Urban Child. By J. B. MAYS. Liverpool: Liverpool University Press. 1962. Pp. xii + 208. 25*s.*

This study of an underprivileged district of Liverpool has as its theme the wastage of scholastic talent and the means of remedy. Thanks to the lucky coincidence of Dr Crawford's carefully designed survey of reading attainment in Liverpool, this wastage is clearly demonstrated. In the chapter which she contributes she points out that although the I.Q.'s of the children within her sample in the districts studied are not significantly different from the city average, they are between a year and 15 months retarded in reading at an average age of 7¾ years. In the underprivileged wards, despite the small difference in test-intelligence, only some 10½ % of the children are selected for grammar schools compared with over 50 % in suburban wards.

Mays argues convincingly that refinements of the selection procedure offer no solution; indeed, if factors such as home support were taken into account this would further prejudice the chances of the underprivileged child and perpetuate his cultural handicap. Curiously, Mays never considers the alternative of a comprehensive type of secondary school.

While recognizing the institutional handicaps—reluctance of teachers to go to 'down-town' schools, out-of-date buildings, large classes—Mays sees this cultural backlog as the heart of the problem. He will have none of the romantic idealization of the traditional working class subculture with its togetherness, hedonism and hand-to-mouth living. In an age when lack of education and industrial skill are likely to bring an increasing risk of unemployment, he asks how the subculture can be adapted to modern economic conditions. He concludes that this can best be done through the schools. But he finds the teachers isolated from the local environment. They do not live in it, and apart from one or two schools contacts with the parents and the home are minimal. Yet the parents look up to the teachers, and are pathetically ready to accept them as social leaders, if only the teachers would accept the role.

An interesting aspect of this analysis is Mays's implicit rejection of the cruder theories of cross-cultural conflict. Teacher-pupil relationships are good; the children are responsive, and there is no trace of resentment or resistance against the imposition of a superior outside culture.

The reader should be warned not to be put off by the first two chapters, which contain little of the essential argument and, in contrast to the remainder of the book, are trite and poorly written. It is a pity also that the type has been so closely set as to make the reading less pleasurable than it might have been.

D. H. STOTT

The Myth of Mental Illness. By THOMAS S. SZASZ. London: Secker and Warburg. 1962. Pp. xiv+337. 35*s*.

The approach to this book should be gradual. Savour the title, then dispel any suspicion that the author is a crank or a born 'anti-psychiatrist' by reading his curriculum vitae on the back cover. Thereafter read the opening chapters quickly until you confirm that here is a Professor of Psychiatry, in America, a trained psychoanalyst, arguing that mental illness is a myth. At this point allow the mind to simmer down and identify your own particular prejudices in favour of and against such an outrageous assertion. When some tranquillity has been achieved follow Szasz carefully.

The argument is that, like many myths, mental illness is a convenient symbolic, magical notion of the cause of certain events, which are essentially moral problems of living. Thus his arguments are aimed at the 'mental illness as a way of life' approach and specifically at the Freudian view. Hysteria is the focus of his attack.

The villains are Charcot and Freud, who made hysteria a quasi-neurological illness and formed a psychopathology round it. In fact, hysteria is a reflexion of problems of living whereby the patient impersonates a sick person 'acts as though he were bodily ill, and invites "treatment" in accordance with the rules of the medical game. Concomitantly, psychiatrists and psychoanalysts...impersonate physicians and play the role of the medical therapist'. Of course we can say that we knew this about hysterics until psychoanalysts told us they were really ill, i.e. their condition had a pathology and could be treated.

This short statement might suggest morbid candour or disillusionment on the part of the author. The argument is, however, more positive. Szasz attributes the belief that hysteria is an illness to a 'rule-following' model of behaviour (shared by the sciences of man) which includes help-giving and help-seeking patterns of behaviour. He offers a wider concept of 'game-playing' as a comprehensive method of mapping the social behaviour of human beings. In this game-playing model 'end-goals occupy a logically (*sic!*) higher level than strategies used in pursuing them'. Thus the theory of personal conduct has to specify norms and values first and techniques of behaviour second. These sentiments would ensure a class-ticket in Moral Philosophy but their significance might elude the professional psychologist (unless he were an ardent McDougallian).

Otherwise for the clinician, though! The hysteric is using childish coercive strategies in pursing an equally childish end-goal of domination and interpersonal control. Failure to bear in mind end-goals cannot be corrected by training in specific techniques of living. Szasz thus takes up an essentially moralist position which the more complacent clinician would reject, particularly in a form such as 'the rewarding of disability...is a potentially dangerous social practice'. This position is deliberately adopted, for psychiatry is nominated as involving a study of personal conduct.

It is perhaps not too difficult to follow the author to his position on the grounds of his analysis of the condition, hysteria. His assertion that hysteria is a paradigm of all mental illness is not convincingly enough argued to sustain his general thesis. Further, the fact that it is written in the setting of contemporary American psychiatric practice makes it too easy for the British worker to reject the argument as irrelevant.

While the thesis, on the face of it, runs counter to corrent psychiatric thought, it is developed from a wide-reaching analysis, involving communication theory, ethics and theory of games—all interesting components. The book could be read with most profit by followers of Barbara Wootton, and with some profit by academic psychiatrists. It might prove too great a deterrent for the psychiatrist in training.

R. M. MOWBRAY

Psychiatry and Responsibility. Edited by HELMUT SCHOECK and JAMES W. WIGGINS. New Jersey and London: D. van Nostrand Co. 1962. Pp. xiv+238. 51s.

It has long been recognized that when psychiatrists step outside the frontiers of individual psycho-pathology to apply their favoured theories and practices in the field of social science, some of them are prone to display not a few foibles, eccentricities and infractions of common sense. For many years music hall artists and caricaturists have held up some of these foibles to good-natured ridicule: but it is only in recent times that organized criticism of alleged errors in social principle on the part of psychiatrists have engaged the attention of, in the main, sociologists and social scientists. Here for example is an omnibus volume whose contributors include three sociologists, two political scientists, one social scientist, two metaphysicians, a historian, a psychologist, a psychiatrist and a member of the American Psychoanalytical Association. Their main concern is to examine the principles of psychic determinism favoured by many psychiatrists. This approach, they feel, may undermine the sense of moral responsibility of the community, threaten the freedom of the creative individual and inhibit the normal functions of conscience.

Two criticisms of this enterprise are relevant. In the first place the title of the book is misleading. In fact it pays very little attention to the varying theories and practices of general psychiatrists; most of the writers concentrate their attention on Freudian and 'neo-Freudian' psychology. And, in the second, the understanding of Freudian psychology displayed by many of the contributors is not of a very high order. The Editors (both sociologists) manifest a vague uneasiness on this point when in their introduction they anticipate that some readers will feel that the positions examined by the contributors are 'straw men'. It would indeed have added considerably to the balance of the book had they included some authoritative presentations of psychoanalytical views on the nature and scope of psychoanalytical determinism. In which case the reader would have been in a better position to judge for himself. The solitary member of the American Psychoanalytical Association does not attempt such a survey, and the solitary psychiatrist divagates from the central thesis of the book to deliver a rousing attack on Freudian concepts and methodology.

To be sure, judged by the standards of their own particular specialties the various contributions are eminently readable and display a considerable degree of dialectic skill. But in so far as they purport to be critics of psychic determinism, the evidence they bring would not pass muster in a court of summary jurisdiction, to say nothing of more scientific panels. In point of fact this volume gives evidence of just as many foibles and eccentricities on the part of the critics as they attribute to the Freudian. In these circumstances the only reasonable verdict to which the general reader can arrive in this particular case is that of 'not proven'.

EDWARD GLOVER

Psychiatry: Biological and Social. By Ian Gregory. Philadelphia and London: Saunders. 1961. Pp. x+577. 70*s*.

Professor Gregory is rightly critical of the verbosity, bias, jargon, woolliness and uncontrolled speculations contained in much current psychiatric literature. He sets out to provide a book 'designed to be concise yet comprehensive, scientific and systematic'. This is a tall order, considering how unscientific psychiatry is in practice.

He is remarkably successful. He lowers his sights at the outset, aiming at 'Biological and Social' rather than 'Clinical' Psychiatry, but he does cover the clinical field in a fairly orthodox fashion. He is at his best away from the specific syndromes, however, amongst considerations of definition, epidemiology, and prophylaxis. His descriptions of treatments and his account of aetiology (the best part of the book) are sound, and there is an excellent chapter on Research. The descriptions of various treatments make these sound a trifle too empirical and unrelated to the clinical problems posed; some of us like to think our efforts are a little more rational, at least in psychotherapy.

It is in the more traditional clinical accounts that the book is less successful. Here, science still contributes but little, and Professor Gregory, like others before him, has to eke out his account with traditional descriptions and with clinical accounts of unspecified objectivity. At times, (notably in the chapter on Marital Problems) he falls back on ordinary subjective clinical wisdom. It is effective, and quite as sound as anyone else's clinical wisdom, but no more scientific.

One defect, in the reviewer's opinion, is the lack of emphasis on distortions of perception and objectivity in the clinician due to normal emotional factors. Chapter 4, in particular, seems to describe ideal rather than real psychiatric behaviour. While it is well to aim for this, a reminder of what actually goes on would not come amiss. This is, after all, the *pons asinorum* in most psychiatric training. Maybe transatlantic psychiatrists are more adequately trained in this respect than we are. Another personal preference would be for rather more about the model psychoses, or at least some reference to recent literature in this field.

In general, this is a sound book, rather conservative, and to be regarded as background reading to clinical experience and not as a work of reference. It makes an excellent introduction to psychiatry, and will enhance the latter's scientific respectability. The balance is good, the references comprehensive, and the quotations well chosen. Its faults are the faults of psychiatry as a whole.

J. AMOR ARDIS

Schizophrenia. By F. J. Fish. Bristol: Wright. 1962. Pp. xii+190. 32*s*. 6*d*.

This book has been compiled from a series of lectures given to candidates for the Dip. Psychiatry and it is intended to be addressed primarily to junior postgraduates beginning their career in psychiatry. Such readers may close the book with grave doubts regarding their choice of career, for the author's brew is indeed a heady vintage. Although Dr Fish modestly describes his work as containing nothing new, there is much here that will be new to many psychiatrists, junior or senior. The emphasis throughout is on the views expressed by German psychiatrists, whose work has been hitherto relatively inaccessible to English readers. Dr Fish's partiality towards the Kleist–Leonhard approach to schizophrenia is made plain in the detailed exposition of their work which occupies much of the text. The recent advances in biochemical and neurophysiological techniques have stressed the need for a more reliable method of classifying clinical subtypes of schizophrenia, but it seems doubtful if Leonhard's highly complex and subjective system as outlined, does much to clarify the issue. The reliability of all such diagnostic systems is likely to remain low until clinical observation is linked to more objective and measurable indices of the patient's performance. Whether or not one can accept many of the ideas expressed, Dr Fish has performed an undoubted service in bringing these views to our notice. There is also much to recommend in the meticulous clinical observation of schizophrenic patients which forms the foundation of the theories outlined in this book.

ANDREW MCGHIE

Variations in Value Orientations. By FLORENCE ROCKWOOD KLUCKHOHN and FRED L. STRODTBECK. Illinois and New York: Row, Peterson. 1961. Pp. xiv + 450. No price given.

Many anthropologists possess, in secret, a various and unpublished collection of field data concerned with values and other motivational categories. These are notoriously difficult to handle, but this book is a brave attempt to deal with inter- and intra-cultural variation in 'value-orientations'—'complex principles which are variable only in patterning'. Five of these, 'common human problems' were selected for study: Human Nature (Evil, Good-and-Evil, Good), Man-Nature (Subjugation-to-, Harmony-with-, Mastery-over-Nature), Time (Past, Present, Future), Activity (Being, Being-in-Becoming, Doing), and Relational (Lineality, Collaterality, Individualism).

From these general categories a more specific multiple choice 'real-life situation' questionnaire was developed in order to test prevailing attitudes in differing cultures and this was given to small samples drawn from five distinct communities, all living within the same 2000 square miles of the American South-West—Zuni, Navaho, Spanish-American, Mormon and Texan. Before this, an 'informal prediction' had been made by the investigators as to the expected rank orders of the value-orientation choices within and between the societies. The evidence on which this prediction was based is given, together with discussion of the results, in the form of ethnographic notes of varying detail and perspective. Sophisticated statistical measures were then applied to the questionnaire results and the inter-cultural predictions received strong confirmation.

A brave attempt—but some doubts about the validity and explanatory value of the findings arise. It seems a pity that the questionnaire was administered by or in the presence of the same people who had made the initial predictions of the results to be expected. Like 'clever Hans' respondents are often capable of 'giving the right answer to inadvertent signs of satisfaction in the investigator. And, in working with communities which vary in number from fifty odd to approximately three thousand persons, one might hesitate to generalize for whole 'cultures from samples of about twenty subjects, even where such samples were 'random'. While the 'value-orientations' may be no really new form of explanation but simply a convenient classificatory scheme for the grouping of ethological variables, the fact that they are couched in general terms and testable by a 'real-life situation' questionnaire, should provide a useful tool for further cross-cultural research.

A well controlled replicative study, with detailed predictions and large samples from widely separated peoples, would be a most fitting sequel to this book.

S. G. LEE

Cognition. By F. H. GEORGE. London: Methuen. 1962. Pp. 309. 32*s*. 6*d*.

Cognition is a well-established and traditional word that is nevertheless uncommon in the titles of psychological books. Its mere use suggests—what is clearly Mr George's intention—an attempt towards the unification of a number of topics (outstandingly, learning, perception and thinking) which have more often been discussed in relative isolation from one another. The book is described as 'principally concerned with tracing the evolution of a liberal behaviourism', a description that could fairly be interpreted in the light of the preceding remark that 'liberal-minded' behaviourists admit that consciousness 'is a problem which we cannot for ever avoid'. The direct discussion of consciousness in the remainder of the book is, however, slight. Cognition is explicitly defined in what Mr George calls the 'behaviouristic way' as referring to unconscious processes as well as conscious, and no special attention is given to possible behavioural correlates of consciousness in specific experimental situations.

A second avowed concern is with the development of a theory language for cognition, and this arises throughout the book. What seems curious in the chapter on theory language is the degree of concern with accuracy of distinction between sensing, seeing and believing in contexts which can only be referred to subjective report or assessment. Though Mr George emphasizes the extreme difficulties attendant upon instruction in perceptual experiments, there is no sustained discussion of the role of instruction in other types of human experiment, although this would seem to be a region where issues of consciousness and meaning conspicuously break through the rigorous experimental surface.

There are doubtless many ways in which Mr George's kind of attempt might be made. First as a general survey in which data from all sectors of cognition are brought together in a novel and suggestive juxtaposition. Secondly, by concentration on those points where disparity of approach or theory are most critical. Or by a comprehensive theoretical attempt (presumably either linguistic or cybernetic) which even by its failure might suggest a different articulation of the field. All three attempts seem to be made with varying degrees of emphasis and success, and there are points at which it is difficult not to feel that a mere deliberately limited range of discussion might have given the book greater force. A good deal of space is given to the specific question of the relationship between the theories of Hull and Tolman, and it is somewhat disappointing that rapprochment is reached in terms of MacCorquodale and Meehl's formalization of Tolman, but with less subtle recognition of its possible deficiencies than is expressed by those authors in *Modern Learning Theory.* A liberal behaviourism of the kind suggested cannot, one would have thought, afford any premature limitation of the issues that Tolman has raised.

ALAN STANILAND

Bodily Sensations. By D. M. ARMSTRONG. London: Routledge and Kegan Paul. 1962. Pp. viii + 132. 12*s.* 6*d.*

This slim volume continues the series of *Studies in Philosophical Psychology* which opened with P. T. Geach's *Mental Acts.* It bears witness to the analytical powers of its author and displays an originality that appears to be lacking in one or two of the preceding volumes.

Those who wonder how, if psychology is an empirical science, there can possibly be a philosophical psychology and not, say, a philosophical botany or geology, will feel more reassured after reading this book. On the other hand, while we may allow that there is a place for philosophical analysis of psychological issues, this does not mean that a licence is granted to philosophers to decide empirical questions. Mr Armstrong's readers may occasionally have the impression that he believes he is equipped with such a licence.

What he does is to discuss in subtle fashion the criteria, logical and psychological, for classifying and differentiating the varieties of sensation, and he succeeds in drawing a number of convincing distinctions, for example, between what he calls 'transitive' and 'intransitive' sensations, that will give the experimental psychologist much food for thought. From this point of view the book is excellent and its author must be congratulated. Nevertheless, there are some reservations. Mr Armstrong acknowledges his debt to Ryle, but this Rylean influence is not one that will necessarily impress the psychologist. For it appears to assume that an empirical examination of the human mind can be founded exclusively on what J. D. Smythies has described as 'an analysis of the grammatical structure of the English language and on what are called "the standards of correct English usage"'. If this assumption were sound, experimental psychology would be unnecessary.

To this I may add that much of what Mr Armstrong has to say about touch and pain is not new. This perhaps matters less than the fact that he does not appear to have familiarized himself with literature highly relevant to his topic. There is no mention of Michotte on the perception of causality, of Révész and Katz on touch in the blind and sighted, nor of Beecher or Buytendijk on pain. I imagine that Mr Armstrong's essay might have assumed a different shape had he steeped himself in the works of these investigators. And I would suggest that the difficulties which Mr Armstrong frequently runs into in relation to so-called 'unfelt sensations' might possibly have been resolved for him had he accepted the distinction between perception and 'prehension', the latter concept being taken not in Whitehead's sense but in the sense of knowing, but not knowing that one knows.

The final conclusions drawn by Mr Armstrong may seem rather disappointing although he claims that they are of great philosophical importance. The book itself, however, is full of interest and could easily provoke the reviewer (if he had the time) to write a review treble the length of the book.

JOHN COHEN

Symbols, Signals and Noise. By J. R. PIERCE. London: Hutchinson. 1962. Pp. xi + 305. 21*s.*

This elementary yet readable introduction to information theory contains 5 chapters on 'the ideas of the information rate or entropy of an ergodic message source, the information capacity of noiseless and noisy channels, and the efficient encoding of messages produced by the source, so as to approach errorless transmission at a rate approaching the channel capacity'. This includes a discussion of the information in printed English, and of Shannon's various orders of approximation to English. The book also contains an excellent chapter discussing the problems in the study of language, grammar and meaning, a chapter on Wiener's Cybernetics, and chapters on the contributions of information theory to psychology and to art. The remaining 5 chapters are probably of greater interest to physical scientists than to psychologists. In addition to a simple explanation in the text when a term is first introduced, the minimal mathematical notation used is explained in an Appendix, and a separate Glossary is provided to explain the technical terms.

The book is a first rate introduction to information theory for the novice, with a wide general appeal. However the very width of Dr Pierce's interests is somewhat of a disadvantage to the student of psychology without much time for background reading. He wants to know what information theory is about, but principally how it is relevant to him; not so much how it is relevant to students in other disciplines. The chapter on information theory in psychology touches rather sketchily upon reaction time and choice, the channel capacity of the human, and Zipf's law. No mention at all is made of the information transmitted by absolute judgements, a field which your reviewer believes to have produced one of the greatest practical successes of information theory in experimental psychology. Thus although the book could be used as an elementary introduction to information theory for students of psychology, some additional reading would be required before the student could appreciate the full impact of information theory upon our particular discipline.

E. C. POULTON

Language and the Discovery of Reality. By JOSEPH CHURCH. New York: Random House. 1961. Pp. xviii+245. $4.00.

One seldom criticizes a book for being too short or too readable. But you cannot put a quart in a pint-pot. Professor Church's theme is of monumental dimensions. His chosen *genre* is little more substantial than a long anecdotal essay. The subtitle 'A Developmental Psychology of Cognition' promises something more architectonic than such a light framework can support. Moreover it is scarcely consistent with the author's own disclaimer towards the end of the book: 'As Merleau-Ponty has said, psychological theories can account for everything except the psychology of their creators. Psychological theorists have been inclined to play God, to see things *sub specie aeternitatis*, egocentrically forgetting that as human beings they can have only a partial and fragmentary and situation-bound command of all the essential phenomena, but also forgetting how their own behaviour transcends their own theoretical formulations.'

'Perhaps it is because of a new relativism, that psychological theory-making in the grand manner has all but disappeared. The psychological landscape is dotted with the ruins of once-splendid theoretical edifices....Eclecticism is the order of the day.'

Yet one very powerful 'order of the day' is Behaviourism and this still carries much of the 'grand manner'. In taking issue with Skinner Professor Church has armed himself with too small a sling and Goliath will not succumb to a peppering of gravel. It is a pity because the book has important things to say. 'Having granted existence to subjective states, Skinner is at a loss what to do with them, so that the conscious person is left hovering helplessly like an ectoplasmic cloud over his own material body. Here we must propose a principle: Reductionism begets Spiritualism.' But having proposed this potentially explosive principle the book ends, not with a bang but a whimper.

Professor Church has an easy, eminently readable style. A command of language is a most welcome gift in a writer on language, by no means universal among such writers. He has an observant eye and, at the level of unsystematized comment, makes many valuable and perceptive interpretations, particularly of the verbal behaviour of children. He makes effective use of his concept of 'schematic mobilization' and shows a generous sensitivity to many aspects of behaviour dealt with by Behaviourists, only with heavy-handed circumlocution, if at all. The eclecticism, which he admits, is all too evident in his sparse coverage of the immense range of linguistic phenomena, and the arbitrariness of the bibliography. The concept of the *schema* having been adopted as central to his theoretical scheme the lack of reference to Oldfield, Zangwill, Wolters and other British contributors is regrettable. This does not, however, as with some transatlantic writers, spring from a wholesale neglect of non-American sources, and one welcomes the reference to William Empson, whose 'Structure of Complex Words' presents many challenges to Behaviourism from a by no means 'tender-minded' poet. But even in such an eclectic treatment as this it is odd to find no reference to Ogden and Richards, Korzybski, Shannon and Weaver or Colin Cherry. Indeed linguistics proper is given scant treatment, logic is no more than glanced at, while the whole mathematical aspect is conspicuously ignored. No single individual can, of course, handle with assurance the full range of disciplines involved in a comprehensive Psychology of Language, and, while regretting the discrepancy between the claims implicit in the title and the substance of the offering, we can still be grateful for a stimulating, at times provocative and always readable series of informal studies on the inexhaustible theme of man's distinctive gift.

PATRICK MEREDITH

Introduction to Semantics. By ADAM SCHAFF. Translated by OLGIERD WOJTASIEWICZ. Warsaw: Państwowe Wydawnictwo Naukowe; London and New York: Pergamon. 1962. Pp. xii + 395. 50*s*.

This book, which refers extensively to Eastern European studies, should appeal to philosophical psychologists and students of communication. Although concerned mainly with logic and linguistics, the subjects covered include anthropology and social psychology. The author is a Marxist who says his purpose is to criticize, but to be more tolerantly and more effectively critical than his predecessors. In this purpose he is successful, making a reasoned and restrained attack on logical positivism that is worth reading. He also gives a useful appraisal of 'general' (i.e. psychological and social) semantics, originated in America by Korzybski, who saw in the way words were used and in their effect on the users ways of alleviating (among other tribulations) disputes, quarrels and neuroses.

When, however, Professor Schaff tries, by means of Marxism, to achieve a reconciliation between transcendental explanations of communication such as that of Jaspers, and naturalistic theories like that of Dewey, he is less convincing, for the reader is required to accept social conditioning as a major source of explanation and not to ask the kind of question which according to the transcendentalists, (and to some extent the naturalists) must be asked, to show how social interaction can occur at all. The author's suggested revision of the typology of signs is also controversial. Are verbal signs *sui generis* as he claims? Can we be sure that language is a precondition of consciousness? Is it justifiable, in referring to Pavlov's second system of signals, to exalt men (as Professor Schaff does) in comparison with animals, emphasizing language and economic production? Do not animals labour, are they not social, do they not in some sense engage in co-operative effort? In this attempt to formulate differentiating characteristics of mankind, materialism and mysticism become entangled.

Similar objections apply (although less strongly) to much of the comprehensive treatment given to meaning and to the communication function of language, but the discussions of Kurt Goldstein's studies of aphasia, of linguistic relativity, and of Benjamin Lee Whorf's *Weltanschauung*, are of general psychological interest. This is a scholarly book and the English translation (checked by George Bidwell) is extraordinarily clear. Footnotes are provided (21 definitions of 'language' are given at one point) and there is a bibliography of nearly 500 references. Regrettably there is no subject-index, only an index of names.

JOHN W. THOMPSON

Mirror of Minds. By GEOFFREY BULLOUGH. London: The Athlone Press. 1962. Pp. viii + 271. 35*s*.

In this expanded version of his Warton Lecture delivered before the British Academy, Professor Bullough seeks to illustrate the ways in which English poetry, from the days of Chaucer onwards, has 'reflected current views of the human mind or soul—its place in the cosmos, its relations with the body, its origin, limitations, and possible survival after death'. As his detailed notes sufficiently testify, he has made a close and systematic study, not only of the numerous poets whom he cites, but also of the philosophers, scientists, or psychologists who were their contemporaries. His account is delightfully written, and his quotations aptly chosen. Quite apart from the suggestive information thus conveyed, the whole discussion provides the student of psychology with a new and fascinating approach to the history of his subject.

Professor Bullough starts by summarizing, in ten propositions, 'the system of psychology inherited by the Middle Ages from Aristotle and Galen, and adapted by St Augustine and St Thomas Aquinas'. But 'under the broad wings of this microcosmic theory' there sheltered, so he tells us, a hotch-potch of common-sense notions, mingled with all kinds of superstitious saws and precepts, and incorporated in a sort of 'folk-psychology'. It is this quaint mixture—with its doctrines of the humours, the four elements, the planetary influences, and its picturesque tales of ghosts, witches, and elves—that we find exploited by earlier writers from Chaucer and Gower to Spenser and Shakespeare, and revived from time to time in symbolic form by later poets like Coleridge, Shelley, W. B. Yeats, and Edith Sitwell. Freud, we are reminded, held that science and civilization had 'banished such primitive fancies to the dream-life or the nursery'; Jung—more correctly, so Professor Bullough implies—maintained that they embody certain 'racial images and archetypal ideas, welling up from the collective unconscious, and typifying recurrent events in the soul's cycle of experience'.

Shakespeare has a chapter to himself. As a dramatist he was 'preeminently interested in the psychology of individuals'. But, unlike Ben Jonson and most of his predecessors, he 'refused to confine his characters within the intellectual strait-jacket' that was furnished by the traditional scheme. Moreover, his views reveal a progressive development from the earlier plays, with their oversimplified theories of motivation, through the complex dramas of his middle period, down to the latest of all where, 'as the whole of the *Tempest* implies, the last word is not with magic or with stoic patience, but with religion'.

During the century that followed the Civil War, there was a marked reaction. The atheism imputed to Hobbes, Gassendi's materialism, the rationalism of Descartes, the empiricism of Locke and Hume can plainly be traced in the poets of the Restoration, and of the 'age of enlightenment' that followed. It culminates in Pope's *Essay on Man*—the poem that is still most frequently quoted by psychologists themselves. To the literary critic it has seemed little more than a cento of moral and philosophical commonplaces; and yet in substance, as Professor Bullough contends, it is a remarkable attempt at a balanced synthesis, seeking to unite in a rational eclecticism the material, the spiritual, and the religious points of view.

With Wordsworth and Blake the pendulum swings once again. And so we come at length to an illuminating discussion of 'associations, intuitions, and immortal longings in the nineteenth century', struggling to combine the optimistic and idealistic outlook of the main poetical tradition with the new discoveries and the disturbing conclusions of physicists, physiologists, and biologists of the day with all their pessimistic implications. Here Tennyson emerges as the poet of mental conflict, Browning and Hardy as the representatives of the opposite extremes and Bridges of the attempted reconciliation.

The book ends with a chapter on 'The Individual and the Racial Image in Modern Poetry'—the longest and for the psychologist the most instructive chapter of all. Professor Bullough's final summary is all too brief. The poet, it is clear, rarely commits himself to any well rounded system of psychology in verse. From the language, the concepts, the current theories of his time, he selects only what serves to body forth his own inner vision. 'What matters is, not the objective truth of his assumptions, but the pervading intensity of their imaginative influence and the precision of his own apprehension of man's place in the scale of being.' CYRIL BURT

Three Essays on the Painting of our Time. By ADRIAN STOKES. London: Tavistock. 1961. Pp. x+65. 12*s.* 6*d.*

This is not an easy book to assess. Apart from the trickiness of his subject and the controversial nature of his approach, the author's style is highly personal, oddly syntacted, oddly punctuated, often obscure and at times baffling: 'Envelopment by building, by street, is almost unknown to Englishmen as a reassurance, but is universal in experiences of confusion or of the drugs that alleviate, such as the droplet comfort in a cottage roof, in a quaint lamp-post or a causeway too windswept for advertisements' (p. 3). But the reader should not be put off by his idiosyncrasies of style: 'Architecture is limited to forms without events; in many styles or periods an architectural exemplar has provided the mode for translating graphic subjects into the terms of a concatenation built upon a ground bass' (p. 4), nor by his blunt assertions. 'The palpable textures of modern painting express the division and disintegration of culture as well as the ambivalent artist's restitution, often carried no further than an assembly of scaffolding' (p. 6).

One cannot avoid the impression that he is wholly committed to psychoanalysis, particularly to the doctrine of Melanie Klein, and appears to approach art via psychoanalysis rather than the converse, which might have been expected from one who is himself a practising artist. The book nevertheless is worth the effort of reading, less perhaps for the light it sheds than for the piquancy of the author's perceptions and observations. Like others who propound a psychoanalytic aesthetic, Adrian Stokes traces art to the primal conflicts of childhood. The spectator, in identifying himself with objects, finds that 'a barrier between self and not self is undone', enabling him to hold commerce with it 'at arm's length'. Self-sufficient objects, of which the work of art is the example *par excellence*, are felt to be enveloping and to contain good and bad components. 'In painting his picture the artist performs an act of integration upon the outside world that has reference, then, as well as to the independence of objects, first to the re-creation and to some resolution of his own inner processes, next in regard to the organisation in the ego in a generalised sense, and finally in regard to a cultural significance'. The basic premise of the book, expressed in the first and perhaps most important of the three essays, is that the work of art is a 'good' object, the counterpart of the 'good' breast; hence its power to overwhelm us, envelop us and our need to incorporate it. The artist 'projects with astuteness upon the canvas an inner need in terms of the outside objects he has chosen, so that both he and they renew life; that is, so as to figure forth a pattern wherein confusion, though it be rehearsed there, may not rule; and greed and sadistic control of the object, though they too may figure, are not unchallenged' (p. 9). The 'good' object must be organized as a pattern: this accounts presumably for the artist's skill. It does not, however, explain—and this is the crux of the whole matter—what differentiates qualitatively one pattern from another and *ipso facto* one 'good' object from another. 'More than the rest of us (the author points out, p. 10) the artist is aware that what we see symbolizes the history as well as the aspiration of the mind: his task is to discover from them a "felicitous embodiment" in the outside world so that they be recognized as any object of perception is known, and better known the better the character of the object or scene represented has been seized in paint'. We need light here on this quality of felicitous embodiment: the author speaks (p. 11) of the work as a 'self-sufficient object as well as a configuration that we absorb or to which we lend ourselves as manipulators' but he offers no clue as to the criteria by which a configuration may be judged good or less good. This is the chief defect of an otherwise searching and provocative book. There can be no doubt that Adrian Stokes is concerned with the fundamental problems of aesthetics; and while he appears to offer an explanation of many aspects of art such as, for instance, the part played by textures in both painting and architecture, he relies largely upon the, as yet, controversial theories of Melanie Klein and other psychoanalysts. Furthermore, there are other psychologies, such as Gestalt theory which offer perfectly viable explanations of the wholeness and integrity of the work of art. One finds, for instance, no mention of the part played by tone in unifying a work of art. The author tells us that 'Cezanne applied a steel-bright knife to pattern and to distance: he introduced love and respect into an extraordinary attack upon his apples and upon the landscapes of his home. His paintings are unified by a play of glinting cuts that both bisect and glorify the contour'. This seems to me to stress only one aspect of Cezanne's work, even granting the author's argument that the cruel power continues in the making of art, 'not only to dismember but to reconstruct what thereof is dismembered'. T. ELDER DICKSON

The Brain as a Computer. By F. H. GEORGE. Oxford: Pergamon. 1961. Pp. viii + 413. 63*s*.

Dr George aims to show how cybernetic models may be applied to behavioural phenomena, thereby rendering more explicit any assumptions made about the underlying processes.

He commences by outlining the foundations of cybernetics in mathematical logic, computer technology, communication theory and servomechanics, leading to the development of the theory of automata and their representation as logical nets. After a discussion of some philosophical and methodological aspects of theory construction, and a brief summary of the logical systems to be used as models, he describes the general properties of finite automata, and gives examples of some models which have been realized in hardware.

This is followed by a more extended treatment of logical networks, especially systems capable of classification, storage, and other functions which seem necessary to models of cognitive behaviour. A chapter showing how computers may be programmed to 'learn' concludes the first part of the book.

The second part attempts to show how specific finite automata may be used to clarify a number of particular psychological problems. The author first gives, for the benefit of non-psychological readers, an outline of the better-known theories of learning and discusses the application of logical nets to several problems in this field. He then gives, also for the non-specialist, a relatively factual survey of neurophysiology, and follows this by a chapter on such theories and models of the nervous system as those of Pavlov, Konorski, and Hebb. The final chapters are concerned with perception and other cognitive processes, and include an account of a model developed by the author.

N. E. LOVELESS

Körperbau und Charakter. By E. KRETSCHMER. Berlin—Gottingen—Heidelberg: Springer. 1961. Pp. xiv+463. DM 39.80.

This latest edition of Kretschmer's magnum opus has not been changed very much from the preceding one, although again new findings and discussions have been incorporated. These relate particularly to relationships with the autonomic nervous system and include the work of Hoff and Schubert. For the rest, although Kretschmer incorporates in his List of References a large number of American and British studies, it cannot be said that he has paid very much attention to the critical points raised by these authors. While, therefore, the book contains much that is suggestive and interesting, particularly in the way of testing psychological traits, it is very difficult to evaluate as a whole. The usefulness of the work is very much reduced by the absence of a Subject and Author Index; it is to be hoped that these will be provided in the next edition.

H. J. EYSENCK

Kleine Charakterkunde. By HUBERT ROHRACHER. Vienna and Innsbruck: Urban and Schwarzenberg. 1961. Pp. viii+299. DM 14. Ö.S. 89.

This is the ninth edition of the book widely used on the continent and translated into Italian, Spanish and Japanese. Its popularity is fully justified; in this very difficult, complex and argumentative field the author is cautious in his conclusions, refuses to follow blindly in one 'school' and retains his critical gifts throughout. His coverage of the literature, including the English and American journals, is wider than is usual in German books, and English readers may discover in this book many references with which they may not have been familiar. The main criticism one may feel tempted to make is the prominence given to Kretschmer's work (55 pages) as compared with Jung's (1 page). It is difficult to believe that this disproportion represents an accurate assessment of the validity or usefulness of these two systems.

H. J. EYSENCK

Der Zulliger-Tafeln-Test (Tafeln-Z-Test). By HANS ZULLIGER. Bern and Stuttgart: Hans Huber. Second edition. 1962. Pp. 290. DM 28.50.

Ich-Diagnostik im Zulliger-Test. By F. SALOMON. Bern and Stuttgart: Hans Huber. 1962. Pp. 316. DM 34.

The *Z*-test was devised by Dr Zulliger, the well-known Swiss psychologist. It is based on the Rorschach test but consists of three new cards instead of the familiar ten. One of the three books Dr Zulliger recommends to his readers is the other book under review, by F. Salomon, a lay-analyst. Each author has about 8000 Z-records. The two books are complementary. Dr Zulliger's book (which is intended as a textbook for beginner and advanced worker!) deals with administration, scoring and tabulation, and with the application of the test to a number of cases, mainly difficult children or adolescents requiring vocational guidance. Charm, wisdom and commonsense inform his assessments and the advice he gives. Salomon's book supplies the theoretical background and his cases are almost exclusively adult neurotics and psychotics.

The technique itself is not new and Klopfer's scoring can easily be substituted for the German symbols used. What is new are the norms for the three cards and the percepts which might be expected from them. The analytical orientation, common to both authors, is much more pronounced in Mr Salomon's work. He is a strict Freudian and his purpose in most cases is to assess the patient's suitability for or response to deep analysis. Within the framework of his orientation Mr Salomon keeps faithfully to the test evidence and his interpretations are penetrating but the emphasis on the stages of sexual development and defence mechanisms makes it often difficult to visualize a coherent personality picture on a behaviourial level. His rationale on Freudian lines applies equally to the Rorschach test. He has a novel approach mainly to shading responses and the meaning of reactions to the various colours. Validation of his hypotheses would be of great interest. His bibliography is not referenced to the text and could be more up to date. Neither of the books has an index.

To use three cards instead of ten is an alluring prospect for the hard-pressed psychologist but he will undoubtedly want to investigate for himself the authors' contention that the Z-test is at least as sensitive as the parent Rorschach.

A. KALDEGG

Psychological Evaluation of the Cerebral Palsied Person. By ROBERT M. ALLEN and THOMAS W. JEFFERSON. Springfield, Ill.: Thomas. 1962. Pp. xiii + 86. $5.

This little book is written by the Professor of Psychology in the University of Miami and the Chief of the Department of Psychology of the United Cerebral Palsy of Miami Rehabilitation Center. After a five-page introduction to the general characteristics of Cerebral Palsy, three chapters are given to intellectual assessment, one to personality assessment, and a final chapter surveys vocational assessment issues.

In the first chapter on intellectual assessment the applications of the Ammons Full-Range Picture Vocabulary Test, the Peabody Picture Vocabulary Test, the California Picture Information Test, the Columbia Mental Maturity Scale, Raven's Progressive Matrices, and the Leiter International Performance Scale are discussed. Apart from the Ammons and Raven's Matrices these tests are, as the authors point out, somewhat short on good normative data.

The second chapter on 'intellectual assessment' in fact considers the Vineland Social Maturity Scale and Gesell Preliminary Behavior Inventory, which are, of course, useful standardized observations, or reported observations, of more or less spontaneous behaviour, which make no attempt to screen off the effects of neurological handicaps. They are therefore the more useful in giving a realistic picture of the child's actual levels of development.

The third chapter on intellectual assessment discusses the adaptive use of the Cattell Infant Intelligence Scale, and the Wechsler Revised Stanford–Binet Intelligence Scale, and the Wechsler Scales, which the authors use less frequently with the 'moderately and severely involved cerebral palsied client' than the preceding tests. After the comments on the Cattell one wonders why it was mentioned at all. In an addendum the Illinois Test of Psycholinguistic Abilities of Kirk and McCarthy, and Frostig, Lefever and Whittlesey's Developmental Test of Visual Perception are mentioned, as a kind of stop-press, but with no indication as to whether the authors have actually used them.

The chapter on Issues and Procedures in Personality Assessment is mostly a short discussion of the literature. The authors' personal experience of formal procedures with cerebral palsied appears to be chiefly in the use of the Rorschach. They deprecate the invention of cerebral palsy personality sterotypes, or their allocation to different types of neurological disability.

The final chapter on Vocational Assessment Issues is excellent. It demonstrated the authors' firm comprehension and humane grasp of what is often the most difficult problem the 'Spastic' has to face, and offers wise advice.

There is a bibliography of 126 titles, of which 20 are by the principal author, 2 in association with his collaborator; and a small index.

The book is well produced but seems rather dear for the 60 pages of letterpress which remain if the reproductions of forms, etc., are deducted.

J. A. L. NAUGHTON

Laboratory Organization and Administration. By K. GUY. London: Macmillan. 1962. Pp. xiv + 386. 50*s*.

This book is primarily intended to cover the course for Laboratory Technician's Work leading to the City and Guilds Certificate. The author is the Chief Laboratory Technician in the Department of Chemistry and Chemical Engineering at the University of Natal, having previously held a similar appointment at the Sir John Cass College, London. The book is written in a rather dogmatic style and the extensive use of short sections with bold subheadings greatly facilitates its use as a source of reference. Practically all aspects of the technician's work in the physico-chemical field are covered. Amongst the eleven chapters the following contain information of particular interest to psychologists, for a number of different reasons: Safety in the laboratory workshop, Optical projection, Laboratory records and technical information, The organization of the work and training of laboratory technicians. The book provides a unique survey of the detailed activities of the laboratory technician in the physico-chemical sciences, and the information about the special topics referred to above contains much that will be useful in the administration of the psychological laboratory and other practical activities connected with teaching. Where relevant, as in the comprehensive treatment of 'Visual Aids' ample illustrations are provided including, for example, cross-sectional diagrams of various forms of projectors. A bibliography of over 200 items is included. Although not exactly matched to the needs of the design and use of psychological laboratories, with its special emphasis on testing people, those concerned will find much to interest them. Since impending developments in 'psycho-chemistry' will depend considerably on layout and procedure connected with the preparation of stimulus material this offers another topic upon which useful information is provided. ROLAND HARPER

L'Année Psychologique. 1961. Parts 1 and 2. Paris: Presses Universitaires de France. NF 20+T.L. each part.

As is customary with this journal, only about a quarter or less is devoted to original papers. The bulk of the space consists of a section on critical notices of articles that have appeared in a wide variety of periodicals followed by a similar section on books. Finally, there are items of current professional interest. In this way of serving its readers, *l'Année* may be among the best in the world, if not *the* best, for keeping them in touch with contemporary development in psychology. As Dr Tajfel remarked in reviewing the volumes for 1960, journals printed in English have no equivalent of this admirable system of comprehensively and systematically surveying the psychological literature, including, in particular, that published in the Soviet Union. One must assume that the formidable organizational problems that this programme entails can only be effective with the aid of an experienced secretariat, which fortunately the editors of *l'Année* seem to have at their disposal.

In the two volumes there are eight principal contributions varying a good deal in length, together with a report by N. Kots taken from a larger publication which appeared in Moscow in 1959. Mme Kots is an investigator distinguished for her studies of primate behaviour. Her report in *l'Année* is a meticulous study of the manner in which a chimpanzee (appropriately named Pâris) makes use of tools; it is admirably illustrated with diagrams and vivid photographs. The name of the inexhaustible Professor Fraisse appears three times, twice as co-author and once alone, and there are two papers by Mme Oléron.

In Part 1, Fraisse and Cl. Voillaume examine experimentally the relationships between the field of apprehension, the level of attention and the alpha rhythm; they draw the conclusion that attentiveness, as subjectively estimated, is associated with blocking of the alpha rhythm but, strangely perhaps, not with the efficiency of apprehension. Mme Oléron continues her earlier researches into the effect of the subject's attitude on his capacity for sensori-motor synchronization; and J. F. le Ny, in experiments on the generalization of verbal stimuli, suggests that conditioning processes enter into 'high-level' activities such as that involved in the prediction of behaviour.

The second volume opens with a report by A. M. Battro and Fraisse on the relationship between the capacity for apprehension and the frequency of eye movements, as the duration of stimulus exposure increases. Records were taken of the number, direction and amplitude of the movements. In general, the results indicate a lack of relationship; and it appears also that frequency of eye movements is independent of the nature of the stimuli employed.

In the next paper, Professor Fraisse confronts an old and unsolved problem: does an increase in frequency of the presentation of a stimulus lead to an overestimation of duration? He employs visual, auditory and tapping situations and finds that the estimate is unaffected by the frequency unless the latter is actually perceived by the subject. An article by F. Jodelet on experimental linguistics adduces evidence that words given in response to a pair of stimulus words are more often of the same semantic category as the latter when these themselves belong to one and the same semantic category. J. P. Boons and P. Bertelson demonstrate that choice reaction is prolonged when the subject is uncertain of the duration of the fore-period. Finally, Mme Oléron, in a novel inquiry, explores the problems which arise when subjects have to reconstruct a 'dismantled' text.

The editors' attention might perhaps be drawn to the fact that the English summaries are not always clear, and sometimes their degree of intelligibility leaves room for improvement. This is a great pity, for the non-French reader thus misses a great deal. JOHN COHEN

Journal of Engineering Psychology. Vol. 1, No. 1 (January 1962) and No. 2 (April 1962). Washington D.C.: Elias Publications. Price not stated.

This is a journal which, according to the statement inside the cover 'publishes original contributions in human factors engineering'. Its first two numbers are certainly a valuable contribution to this field. The eight papers which form the two issues report experimental studies on (1) the recovery of ability to read instruments in red and white light after brief bright flashes; (2) a thoughtful article on the effects of noise on the transmission of complex messages: (3) the speed and accuracy of identifying signals consisting of long and short flashes of light; (4) effects of exposure time and speed on the ability to distinguish Landolt rings viewed on a moving target; (5) the readability of upper and lower case letters of an alphabet with uniform stroke width; (6) the advantages of supplementing form by brightness, colour and flashing as methods of encoding signals; (7) the use of a simulated 'roadway' display when steering submarines by instruments; and (8) effects of change-over from one operator to another in a simulated air traffic control task.

Many of the articles are clearly reports of contract work which all too often at present are available only in duplicated form and are not readily available to would-be readers. They come from some of the university, service and commercial laboratories most respected for this work in the U.S.A. and, in most cases, well represent the type of applied study with fundamental implications which has become justly influential since the war. For this reason the journal deserves a place in any library which attempts to cover the field of experimental psychology on the human side.

The numbers are modest in size—46 pages each—but they are well produced and attractively printed. A minor criticism is that the authors' addresses are not always sufficient for the reader to be able to correspond with them. The texts also contain some errors and infelicities which are to be regretted unless the editor (Lee Cozan) is taking the bold step of punishing careless authors by publishing articles exactly as they are received, thus leaving their faults in the full view of posterity. A. T. WELFORD

Readers' Guide to Books on Psychology. 2nd Edition. (New Series, No. 69.) London: Library Association County Libraries Group. Pp. 40. 2*s.* 6*d.*

This new edition is 'a select list of works published in Great Britain to the end of 1961'. The entries are classified under 34 alphabetically arranged headings. Inclusion under one or other of these is—inevitably—often arbitrary; a broader classification, supplemented by an author index, might have been preferable. Excluded are 'unscientific and popular books' and 'works on applied psychology'. Some of the section headings are unexpected, both by their inclusion and in their scope: thus, under 'Occultism' one finds, mainly, a selection of reputable books on ESP. There is no section on Animal Psychology, nor one on Perception, though the latter term appears in the titles of five out of the six books listed under 'Sensation'. Medical psychology, in all its branches, is covered well, and educational and other applied fields are better served than the disclaimer noted above would suggest. B. SEMEONOFF

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CLASSIFICATION AND QUANTITATIVE JUDGEMENT

By HENRI TAJFEL and A. L. WILKES
Department of Social and Administrative Studies, University of Oxford

The investigation is concerned with the effects on judgement of some relations between the manner in which stimuli of a series are classified and the magnitudes of the stimuli. It is shown that when the classification stands in a direct and predictable relation to a physical scale, the stimuli belonging to different classes are judged as farther apart on that scale than in an unclassified series. A classification which is not coherently related to the physical scale does not affect judgement in this manner.

There is also evidence in the results that, as a function of past experience with the classification, an increase occurs in the apparent differences between stimuli belonging to different classes, and in the apparent similarity of stimuli belonging to the same class.

The relevance of these findings to the general problem of stereotyping is discussed.

1. Introduction

The purpose of the present paper is to report experiments designed to test some of the hypothesis put forward in a previously published paper (Tajfel, 1959*a*). The principal trend of the argument in that paper was that shifts and biases in stereotyped judgements can be subsumed under similar shifts existing in absolute judgements of series of physical quantities. This is best shown when one takes into account the effects on judgement of those aspects of judging situations that cannot be included in the description of the physical properties of the stimuli. Examples of such 'non-physical' determinants of judgement are: the relevance to the subject of differences between the stimuli in a series (e.g. Bruner & Tajfel, 1961; Tajfel, 1957; Tajfel, 1959*b*; Tajfel & Cawasjee, 1959); the subject's perception of some stimuli as relevant or not relevant to his judgement of other stimuli (e.g. Davidon, 1960); and the manner in which a series is classified by the subject.

Most work on psychophysical phenomena has been concerned with the formulation of functions relating orders of physical magnitudes to series of corresponding discriminative responses. The existence of predictable relations between these two orders of events does not preclude the possibility of existence of second-order laws which would state coherent and predictable relations between judgement and some subjective aspects of the judging situation. These second order laws would be most directly relevant to prediction of shifts and biases in social situations. However, they should first be established through investigations of scales of response pertaining to series of physical magnitudes in simple and well-controlled judgement tasks.

The experiments reported here are concerned with classification as a variable. A series of stimuli may be classified in a number of ways, and there can be a number of possible relations between a classification and the physical magnitudes which are subjected to judgement. A series of stimuli may be classified in terms of a discontinuous attribute (e.g. a group of people classified into, say, Swedes and Italians); the dimension along which the judgements are made (e.g. height) may be a continuous one. If in our group of people *all* the Swedes were taller than *all* the Italians, there would be a perfect correlation between the classification and the physical attribute under judgement, though the series was not classified in terms of that attribute. If our group of people consisted of, for example, French and Italians, and there was no difference between the means and the spreads of the heights of individuals in both groups, we would have a classification standing in no relation to the physical attribute under judgement.

These two imaginary cases may be considered as two extremes of a continuum, ranging from an entirely predictable relation between a classification and a physical dimension to a complete lack of relation. Most real life cases would fall, of course, between the two extremes; though some (e.g. skin colour and some racial classifications) approach very nearly to a perfect correlation, and many could be found at the uncorrelated end of the continuum.

The problem with which the present experiments are concerned can be stated as follows: how does the subject's knowledge that stimuli fall into various classes affect the judgements of a physical dimension of the stimuli when this dimension stands in a certain correlation to the classification?

The predictions stated in the paper previously referred to (Tajfel, 1959*a*) were as follows:

'(i) When a classification in terms of an attribute other than the physical dimension which is being judged is superimposed on a series of stimuli in such a way that one part of the physical series tends to fall consistently into one class, and the other into the other class, judgements of physical magnitudes of the stimuli falling into the distinct classes will show a shift in the directions determined by the class membership of the stimuli, when compared with judgements of a series identical with respect to this physical dimension, on which such a classification is not superimposed.

'(ii) When a classification in terms of an attribute other than the physical dimension which is being judged is superimposed on a series of stimuli, and the changes in the physical magnitudes of the stimuli bear no consistent relationship to the assignment of the stimuli to the distinct classes, this classification will have no effect on the judged relationships in the physical dimension between the stimuli of the series' (pp. 20–21).

The effects predicted in paragraph (i) above depend on the information which the classification offers regarding the differences between the stimuli. Each stimulus is judged individually, and each has a label identifying it as belonging to one or another of two classes. In the subject's past experience, the stimuli belonging to the distinct classes differed consistently from each other along the quantitative dimension which he is judging. The effect of this situation on judgement can be stated in terms borrowed from the experimental literature on anchoring.

An anchoring stimulus placed outside a series of stimuli may determine either a contrast or an assimilation effect in the judgements of the stimuli of the series. In the case of contrast, judgements of the stimuli shift away from the value of the anchor stimulus; in the case of assimilation they shift towards that value. There exists a good deal of evidence that when the anchor stimuli are placed relatively far outside the original series, they determine a contrast effect; when they are placed very near or at the extremes of the series, they often lead to an assimilation effect.

It is a fair assumption that the judgemental process underlying the contrast and assimilation effects reduces itself to the perceived extent of similarity and of difference between the anchoring stimuli and the stimuli of the series. When the anchoring stimulus is perceived as definitely different from those nearest to it in the original series, the judgements of these stimuli reflect this differentiation by shifting away from the judged value of the anchor; when the anchoring stimulus is perceived as very similar to the stimuli of the series, the opposite happens: the judgements of the stimuli nearest to the anchor reflect the subjective equivalence by shifting towards the judged value of the anchor stimulus.* This would fit in with the general results of experiments on anchoring which show that when the values of anchoring stimuli progressively approach the values of the end stimuli of the original series, the extent of contrast is reduced (e.g. Heintz, 1950) until finally assimilation takes over (Sherif, Taub & Hovland, 1958).

A classification superimposed on a series of stimuli may be regarded as determining the same type of shifts in the extent of judged similarities and differences between the various stimuli. The nature of these shifts will depend on the relationship between the division of the stimuli into classes and the pattern of variation in the magnitudes of the stimuli which the subjects are requested to judge.

In a series in which a classification is directly and consistently related to the physical dimension under judgement, the class identification of a stimulus provides a supplementary source of information about the relationship of its magnitude to the magnitudes of other stimuli, whether identified as belonging to the same class or to a different class. In absolute judgements upon a series of stimuli which are presented in successive random orders, an identity in the labelling should therefore produce assimilation or convergence in the assessments, whereas a difference should produce contrast or divergence. Therefore, the differences between the judgements of magnitude of stimuli belonging to the distinct classes should in such a series be greater than in an identical series on which a classification has not been superimposed. At the same time, differences between the judged magnitudes of stimuli belonging to the same class should show a tendency to be smaller than the corresponding differences in an unclassified series.

There is no reason to assume that any consistent shift in the differences between the judged magnitudes of stimuli belonging to distinct classes should be observed when a classification superimposed on a series stands in no coherent relation to the magnitudes of the stimuli. In this situation, 'the physical magnitude of a stimulus does not help in its assignment to one of the various classes, and the identification of a stimulus as belonging to one of the classes does not provide any cue as to its physical magnitude' (Tajfel, 1959*a*, p. 23).

Two subsidiary hypotheses concerning the effects on judgement of a consistent relationship between a classification and physical magnitudes can be briefly stated as follows:

(i) The predicted effects of an increase in the differences between the judged magnitudes of stimuli belonging to the different classes and of a decrease in such differences between stimuli belonging to the same class should become accentuated as the classification is made more salient to the subjects. The term 'salience' is used here in a restricted sense. A classification may become more salient to a subject because of an increase in the amount of past experience with it; or because of its unvarying consistency. In the present experiments salience was manipulated by presenting the subjects with varying conditions of visual impact of the classification.

---

* This assumption was independently validated in experiments (Tajfel & Wilkes, in preparation) in which the relative extent of contrast was seen to depend on the manner in which the anchoring stimuli were classified in relation to the classifications imposed on the stimuli of the original series.

(ii) These effects should also become accentuated in direct relation to the amount of past experience in judging the classified series.

II. METHOD

The purpose of the experiments was to assess the effects of classifications on simple quantitative judgements. Judgements of length of a series of lines were used. This choice was dictated by the relative simplicity of judgements of length; if it is found that classification exercises its predicted effects in this context, that 'stereotypes' can be formed about the length of lines, then such effects are likely to be even more marked when the complexity of the task of judging increases, with the corresponding tendency of the subject to rely increasingly on all the available sources of information about the stimuli.

Two pilot experiments showed that the series of stimuli described below was clearly discriminable in an absolute judgement situation, and that the effects of classification clearly tended in the predicted directions.

Stimuli

A series of eight lines differing from each other by a constant ratio of approximately 5 % of length were used as stimuli in all the experiments. The shortest line was 16·2 cm. long; the longest 22·9 cm. Each line was drawn diagonally on a sheet of white cardboard whose dimensions were 63·5 cm. × 50·8 cm. The relatively large size of the sheets of cardboard was chosen in order to minimize the cues about the differences in the length of lines that could be provided by comparisons with the frame. Each length of line was presented several times (see below), but a separate sheet was prepared for each presentation of each stimulus, in order to avoid the possibility of an identification of a stimulus on the basis of irrelevant cues.

Subjects

Sixty-one subjects took part in the first two experiments (Expts. I*a* and II*a*). They were volunteers, men and women, drawn from students at Oxford University, Westminster Training College, and Manchester University. Of those sixty-one subjects, fifty-four were available for the second sessions (Expts. I*b* and II*b*).

Procedure

(i) *Procedure common to all experimental situations*

The eight lines were presented one by one six times in successive random orders. The subjects were requested to judge the length of each line in centimetres. Judgements in centimetres rather than inches or fractions of inches were used because in this way the differences in length between the stimuli could be made fairly small while avoiding at the same time the tendency for rough judgements in terms of the nearest inch. Before the beginning of judgements, a ruler was shown and several examples of relationship between centimetres and inches were given. The subjects were tested individually. They were seated facing the stimuli which were presented by the experimenter at a distance of about 8 ft. They called out their judgements which were recorded by the experimenter. There was no time limit for the presentation of a stimulus which was held in front of the subject until the judgement was made. The interval between the presentation of successive stimuli was 4 sec. The subject was not told how many different lengths of lines were presented to him. Each subject was requested to come for a second session a week after the first.

Table 1 presents the summary of the procedures used in the four experiments.

Table 1. *Summary of the experimental procedure*

Experiment	Groups*	Mode of prior presentation	Number of presentations of the series of stimuli
I*a* (first session)	C, R, U	Successive	6
I*b* (second session)	C, R, U	Simultaneous	11
II*a* (first session)	C_1, U_1	Simultaneous	6
II*b* (second session)	C_1, U_1	Simultaneous	6

* C and C_1, classified series; U and U_1, unclassified series; R, randomly classified series.

(ii) *Classification*

Three groups of subjects served in Expts. I*a* and I*b*, and two groups in Expts. II*a* and II*b*.

Experiment Ia. Group C (classified). This group was presented with a classification superimposed on a series in such a way that there was a stable and predictable relationship between the lengths of lines and their labels. Each of the four shorter lines had a large letter A drawn above its middle at each presentation; each of the four longer lines a letter B drawn in the same way. The group consisted of twelve subjects.

Group R (randomly classified). This group was presented with a classification which bore no relationship to the lengths of lines. The stimuli were labelled with letters A and B as for group C; but each of the eight stimuli was labelled A at half of its presentations and B at the other half. The order of appearance of labels A and B was randomized for each stimulus. The group consisted of thirteen subjects.

Group U (unclassified). Conditions of presentation of stimuli for this group were identical to those for groups C and R, the only difference being that the lines were presented without any labels. The group consisted of twelve subjects.

Experiment Ib. A week later, the subjects in groups C, R and U were tested again. The conditions of presentation of stimuli were the same for each group as in Expt. I*a*. In group C, ten out of the original twelve subjects were still available; all the thirteen subjects from group R reappeared; and eleven out of twelve from group U.

Experiment IIa. Group C_1 (classified). Conditions of presentation of stimuli were the same as for group C in Expt. I*a*. The group consisted of twelve subjects.

Group U_1 (unclassified). Conditions of presentation of stimuli were the same as for group U in Expt. I*a*. The group consisted of twelve subjects.

Experiment IIb. The same subjects were tested again a week later. The conditions of presentation of stimuli were the same for each group as in Expt. II*a*. Eleven out of twelve subjects were still available for group C_1, and nine out of twelve for group U_1.

(iii) *Salience of the classification*

All the subjects in all the experiments were familiarized with the stimuli prior to their presentation for judgement. They were told that the lines shown to them before the judgements were representative of those they would have to judge subsequently.

One kind of procedure for this prior presentation was adopted in Expt. I*a*, and a different one in Expts. I*b*, II*a*, and II*b*. In Expt. I*a*, the series of eight lines was presented twice, each line separately and in succession. The first presentation of the series was done in the following order: the shortest of the longer stimuli (those in class B) was presented first, followed by the other stimuli of that class in order of increasing length. The longest stimulus B was followed by the shortest stimulus of the class A followed in turn by the other stimuli of that class in order of increasing length. This was reversed in the second presentation of the series in which first the shorter stimuli (class A) were presented in order of decreasing length, followed by the longer stimuli (class B) presented in the same order, so that again the shortest stimulus of class A was contiguous with the longest stimulus of class B. It was hoped that this introduction of sharp contrast between the A's and the B's would help to convey the idea of the classification to the subjects. The same order of prior presentation was adopted for the three groups of subjects. For group C, the stimuli were labelled in the same manner as in the subsequent presentations for judgements, with labels A for the four shorter lines and labels B for the four longer lines. For group R, the labels alternated randomly along the dimension of length. For group U, the lines were presented without labels.

In Expts. I*b*, II*a* and II*b* prior presentations of stimuli were made simultaneously, not successively. The sheets of cardboard containing the eight lines of different length were displayed on long tables in such a way that the longest stimulus of class B was placed next to the shortest stimulus of class A. The labels followed the same pattern as in the subsequent presentations for judgement, i.e. they were related to length for groups C and C_1, unrelated to length for group R, and no labels appeared for groups U and U_1. The subjects were allowed to walk around and look at the lines for as long as they wished.

In the experiments with groups C and C_1, in which the stimuli to be judged were explicitly classified according to length, it was assumed that a simultaneous presentation would render the correlation between lengths and classes far more obvious to the subjects than a successive presentation. Groups C and C_1 differed in the extent of salience for their first sessions; for their second sessions, in which they had the same condition of simultaneous prior presentation, they differed in the extent of salience built into their past experience of the classification.

(iv) *Past experience*

(*a*) *Effects of past experience at successive sessions.* As each group of subjects underwent two experimental sessions at an interval of a week, the data allowed an assessment of the cumulative effects of classification on judgements of length.

(*b*) *Effects of past experience within a session.* This was investigated in Expt. I*b*, which was, it will be remembered, the second experimental session for groups C, R and U. After these three groups had completed their judgements of the series of stimuli presented six times, the series was presented without interruption in five additional successive random orders, identical for all groups, and identical in their respective conditions to the previous six presentations for each of the groups. Thus, the effect of past experience within a session was assessed by comparing the judgements of the first six with the last five presentations of the series in this experiment.

III. RESULTS

(i) *Description of the general pattern of results*

Table 2 sets out the general results: means of all the control groups (randomly classified and unclassified) were combined, as their patterns are highly similar. The table shows the means of the judgements of each stimulus. Stimuli have been numbered from 1 to 8 in order of increasing length. Stimuli 1 to 4 belong to class A, stimuli 5 to 8 to class B in the classified series (groups C and C_1). Therefore, the *inter*-class difference is between stimuli 4 and 5. All the other differences between adjacent stimuli are *intra*-class differences. The data from Expt. I*b* include only the first six presentations of the series.

Table 2. *Mean judgements of stimuli in the various experimental conditions*

	Class A				Class B			
Stimuli	1	2	3	4	5	6	7	8
Actual values	16·2	17·0	17·9	18·8	19·7	20·7	21·7	22·8
	Expts. I a and II a (first sessions)							
Groups C and C_1	16·0	17·3	18·1	19·3	21·1	22·3	23·6	25·3
Groups R, U and U_1	16·4	17·3	18·2	19·3	20·3	21·5	22·6	24·2
	Expts. I b and II b (second sessions)							
Groups C and C_1	15·6	16·5	17·2	18·3	20·3	21·6	22·4	24·4
Groups R, U and U_1	16·6	17·4	17·9	19·0	20·3	21·3	22·8	24·6

In Figs. 1 and 2 the actual difference between each two adjacent stimuli was taken as a basis, and the deviations of the judged differences from the actual ones are expressed as percentages of the actual differences. These deviations are plotted as positive when the judged differences are greater than the actual ones; as negative when they are smaller.

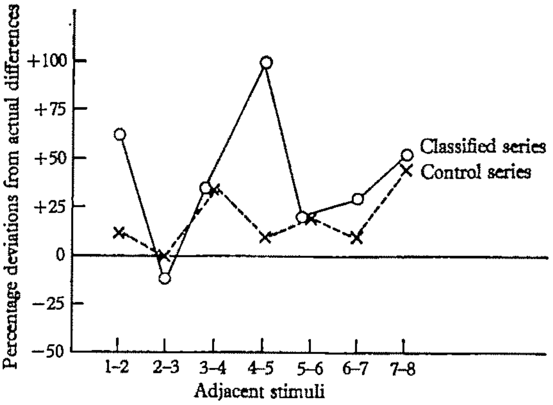

Fig. 1. Comparison of actual and apparent differences between adjacent stimuli in Expts. I*a* and II*a*.

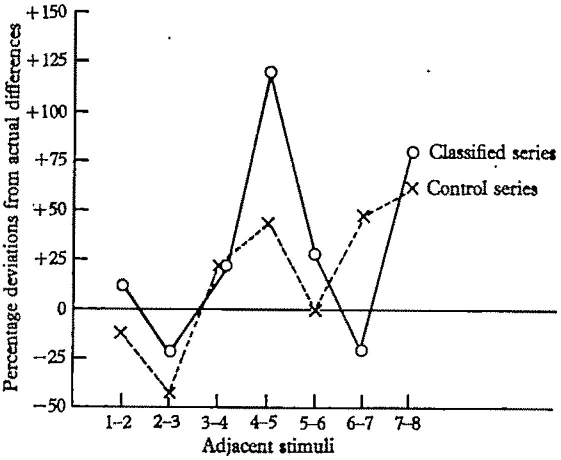

Fig. 2. Comparison of actual and apparent differences between adjacent stimuli in Expts. II*a* and II*b*.

An inspection of Figs. 1 and 2 will show that, as predicted, the inter-class differences (between stimuli 4 and 5) are by far larger for the classified series (groups C and C_1) than for the randomly classified and unclassified ones, both in the first and in the second sessions. In the first sessions the relevant percentage figures are as follows: apparent difference 100 % greater than the actual difference for the classified series; 11 % greater for the randomly classified and unclassified ones. In the second sessions: 122 % greater than the actual for the classified series, 44 % for the unclassified ones. This represents, of course, a considerable exaggeration of the actual inter-class difference for the classified series. There are two other points at which a difference between two adjacent stimuli is markedly exaggerated for these groups. The first is between the stimuli 7 and 8; but this seems to be a consistent phenomenon for all the groups of subjects and is therefore probably inherent in the nature of the range of stimuli and in the pattern of presentation. The differences between the experimental and control groups are very small as compared with corresponding differences at the point of the inter-class break.

The second point at which the intra-class differences are considerably larger than the actual differences for the classified series is between the stimuli 1 and 2 in the first sessions for groups C and C_1. At this point, the difference is not only larger than the actual one, but also larger than the differences in the control groups (the percentages are: apparent differences 62 % greater than the actual differences for the classified series; 13 % greater for all the control series). The direction of this relationship between the classified and control series is maintained in the second sessions (see Fig. 2). In the classified series the apparent difference between the stimuli 1 and 2 is 13 % greater than the actual difference; in the control series the apparent difference is 11 % smaller than the actual one. This problem will be dealt with in the section devoted to the discussion of results.

(ii) *Analysis of results*

It was assumed in the Introduction that a stable and predictable relationship between a classification and a variation in physical magnitudes would lead to an increase in the apparent differences between the classes, and a decrease in the apparent differences within the classes. Therefore, an analysis of results was required which would take into account the combined effects on judgements of both these predicted phenomena. In order to do this, the judgements of stimuli belonging to each of the classes were treated separately, and a linear function was fitted to the judgements of the stimuli from each of the classes, independently of the other. This was done for the judgements of each subject in all the groups. The method of averages was used. The calculations yield the following scores: m_s, the slope for the judgements of stimuli belonging to the shorter class; m_l, the corresponding slope for the longer class; Y_4, the derived value for the judgement of the longest stimulus of the shorter class; Y_5, the derived value for the judgement of the shortest stimuli of the longer class.

Thus, the m_s and m_l slopes provided direct measures of the apparent differences between the stimuli of the same class, the intra-class differences. The difference between the derived values Y_4 and Y_5 provided a measure of the apparent inter-class difference. This last measure is, of course, affected both by the difference between the judged values of the stimuli 4 and 5 at which the break between the classes occurred, and by the intra-class slopes. It would increase as a function of an increase in the judged differences between the stimuli 4 and 5 and of a decrease in one or both of the intra-class slopes, and it would decrease in the opposite case. It fulfils, therefore, the conditions for testing the combined predicted effects of the classification. At the same time, the separate contribution to the results of the intra-class differences could be assessed by a statistical analysis of the slope indices.

Classification and its salience. The analysis of results in relation to the effects of the classification and of its salience was conducted in the following separate stages:

(*a*) A 2 × 2 analysis of variance using the groups C and C_1, U and U_1 in first sessions (Expts. I*a* and II*a*). This allowed an assessment of the effects of classification and of salience of classification (simultaneous *versus* successive prior presentation) for the classified *versus* unclassified series.

(*b*) A 2 × 2 analysis of variance using the groups C and C_1, U and U_1 in second sessions (Expts. II*a* and II*b*). This allowed an assessment of the effects of repeated classification and of differences in the extent of its salience in the past for the classified *versus* the unclassified series. (It will be remembered that Expts. I*b* and II*b* did not differ in the manner of the prior presentation of stimuli, which was simultaneous in the case of both experiments. However, subjects in Expt. I*b* had previously experienced a successive prior presentation; those in Expts. II*b* a simultaneous one. See Table 1.)

(*c*) Only Expts. I*a* and I*b* included groups of subjects judging the randomly classified series. This asymmetry in the design (mainly due to difficulties in finding sufficient numbers of available subjects at the time) made it impossible to include these groups in the previous analysis. Separate Mann-Whiting U tests were therefore conducted comparing the performance of the group judging the classified series with the group judging the randomly classified series. The following comparisons were made: (i) group C *versus* group R in Expt. I*a*; (ii) group C *versus* R in Expt. II*a*. The main prediction of relatively greater inter-class differences for group C than for group R was the same as for the comparisons between the classified and the unclassified series.

Table 3. *Mean inter-class differences and intra-class slopes*

Groups	Inter-class differences $(Y_5 - Y_4)$	Intra-class slopes
Expts. I*a* and II*a* (first sessions)		
C and C_1	1·9	1·22
U and U_1	1·1	1·18
R	1·1	1·06
Expts. I*b* and II*b* (second sessions)		
C and C_1	2·1	1·08
U and U_1	1·4	1·17
R	1·1	1·01

Table 3 provides the group means of the scores relevant to the analysis, namely: mean intra-class slopes $\frac{1}{2}(m_s + m_l)$ and the inter-class differences $(Y_5 - Y_4)$.

The results of the statistical analysis were as follows:

(*a*) The analysis of variance for the inter-class differences $(Y_5 - Y_4)$ in Expts. I*a* and II*a* shows that the classification, as predicted, determines an increase in the apparent inter-class difference (F ratio for the classification is 6·12: $P < 0{\cdot}025$). Differences in the extent of salience (manner of prior presentation) do not lead to any changes in the apparent inter-class differences.

(*b*) The analysis of variance for mean slopes $\frac{1}{2}(m_s + m_l)$ shows that neither the classification nor its salience determined any consistent differences in the apparent intra-class differences This does not confirm the prediction that an introduction of the classification would lead to an increase in the judged similarity of stimuli belonging to the same class.

(*c*) Analysis of variance for Expts. I*b* and II*b* shows results parallel to the previous analysis. The predicted effects of classification for inter-class differences are significant (F ratio $= 4{\cdot}40$; $P < 0{\cdot}05$). No other differences between the groups are significant.

(*d*) A comparison of apparent inter-class differences between groups C (classified) and R (randomly classified) in the first session (Expt. I*a*) yields a strong tendency for the differences in group C to be larger, as predicted; but this tendency does not reach statistical significance. In the second session (Expt. I*b*), this difference between the groups is significant in the predicted direction at $P < 0{\cdot}01$. A comparison of the two groups taking as a score for each subject the means of his performance in the first and second sessions is significant at $P < 0{\cdot}025$ in the direction of larger inter-class differences for group C. There are no differences between the intra-class slopes of the two groups, either in the first or in the second session.

Effects of past experience

The possible cumulative effects of classification as function of past experience from first session to the second were assessed by comparing the relevant scores in Expts. I*a* and II*a* with those in Expts. I*b* and II*b*. The effects of cumulative experience with the classification within a session were assessed by comparing the scores in the first six presentations of the series in Expt. I*b* with those obtained in the last five presentations in that experiment.

(*a*) *Comparisons of first and second sessions.* A 2 × 2 analysis of variance was conducted on the shifts of the inter-class differences from Expts. I*a* to I*b* and II*a* to II*b* for groups C, C_1, U and U_1. Neither the effects of past experience alone (Expts. II*a* to II*b*) nor the effects of past experience combined with an increase in the salience of the classification (Expts. I*a* to I*b*) are significant. The same is true of the analysis of intra-class slopes, and of a separate comparison made of the shifts from first to second sessions in inter- and intra-class differences for groups C and R (Expts. I*a* to I*b*).

(*b*) *Effects of past experience within a session.* It will be remembered that in Expt. I*b* there were three groups of subjects (C, R and U) who after having completed their judgements of the series presented six times, judged the length of lines in five additional presentations of the series. Table 4 sets out the group means obtained from these additional presentations. Fig. 3 presents the percentage differences between the actual and apparent differences for judgements of each two adjacent stimuli; these percentages have been plotted as positive when the apparent differences exceeded the actual ones; as negative in the opposite case.

As can be seen from Table 4 and from Fig. 3, it is only at the predicted point of inter-class break (stimuli 4–5) that the apparent difference between the stimuli for group C exceeds considerably both the actual differences and the corresponding differences for the two control groups.

Linear functions were fitted to these data in the manner previously described. Group C was then compared separately with group R and with group U, both for its derived inter-class differences ($Y_5 - Y_4$) and mean intra-class slopes $\frac{1}{2}(m_s + m_l)$. The two control groups R and U do not differ from one another (see Table 5); the inter-class differences in group C are significantly larger than those in group U at $P < 0{\cdot}01$; than those in group R at $P < 0{\cdot}001$.

At the same time, the intra-class slopes show a tendency to be less steep in group C than in the two other groups; but this tendency does not reach statistical significance when group C is compared with each of the two control groups separately.

An inspection of these results shows indirectly the accentuated effects of the classification as function of repeated trials within the same session. A direct assessment of these cumulative effects of classification was made by calculating for each subject the shift of his scores for inter-class differences and for intra-class slopes from the first six to the last five presentations of the series. The statistical significance of the difference in these shifts between group C and each of the control groups was assessed separately. An inspection of the data in the second part of Table 2 and in Table 4 will show that, as function of practice within a session, apparent differences between the stimuli tended to decrease with the only clear exception of the inter-class difference in group C. The stability of the inter-class differences in group C within the session

Table 4. *Mean judgements of stimuli in the last five presentations of the series in Expt. Ib*

	Class A				Class B			
Stimuli ...	1	2	3	4	5	6	7	8
Actual values	16·2	17·0	17·9	18·8	19·7	20·7	21·7	22·8
Group C	17·1	17·9	18·6	19·1	21·4	22·0	22·8	24·2
Groups R and U	16·9	18·1	18·6	19·8	20·4	21·2	22·2	23·8

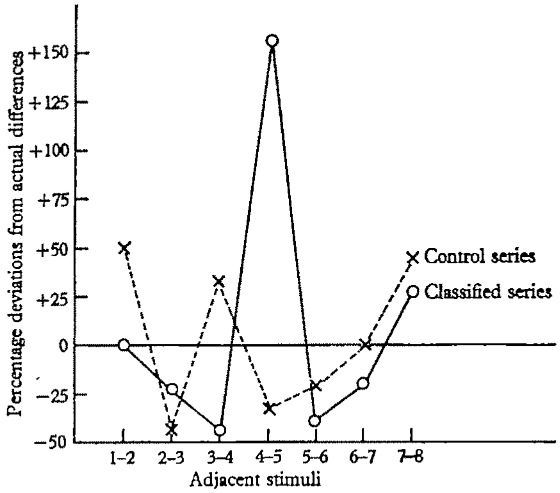

Fig. 3. Comparison of actual and apparent differences between adjacent stimuli in the second part of Expt. I*b*.

Table 5. *Mean inter-class differences and intra-class slopes in the last five presentations of the series in Expt. Ib*

Groups	Inter-class differences $(Y_5 - Y_4)$	Intra-class slopes
C	2·0	0·83
R	0·7	0·99
U	0·9	1·02

as compared with the decrease of the corresponding differences in the control groups does not quite reach statistical significance at $P = 0{\cdot}05$. The decrease in the intra-class differences (i.e. the flattening out of the intra-class slope) compared with the corresponding decrease in each of the two control groups is significant in both cases at $P < 0{\cdot}01$.

IV. DISCUSSION AND SUMMARY OF RESULTS

Before the results can be summarized and conclusions drawn, certain problems of control arising from the data and from the experimental procedures must be briefly discussed.

The results concerning the effects of classification on judgement cannot be attributed to extraneous factors irrelevant to the experimental variables:

(i) The mean scale values of the classified (C and C_1) and control (U, U_1 and R) groups were compared; there are no significant differences between the experimental groups and any of the control groups.

(ii) Variability scores for the judgements of each stimulus by each subject in all the groups were calculated, and comparisons of mean scores made. There are no significant differences between the experimental and the control groups.

(iii) An inspection of Fig. 1 shows that there is a marked difference between groups C and C_1 and the three other groups not only at the predicted point of inter-class break (stimuli 4–5) but also, to a lesser extent, between stimuli 1 and 2. An analysis of variance, parallel to the one conducted on the inter-class differences, was done on these data. There is not even a hint that the difference between the experimental and the control groups is statistically significant (the F ratio for the classification variable is 0·47). It seems therefore safe to conclude that the only consistent difference between the experimental and the control groups is at the predicted point of the inter-class break.

(iv) There was a possibility that the larger inter-class differences found in all the experiments for groups C and C_1 were due not to the effects of classification but to some measure of differential over- or under-estimation in one direction or another inherent in the use of letters A and B as labels. This could be checked by inspecting the data from group R where these two labels were used in random alternation for each of the stimuli. The mean judgements of each stimulus were calculated separately when it was labelled A and B. They are practically identical; the mean scale value for label A is 19·5; for label B $-19{\cdot}6$. The same applies to the mean differences between the judgements of adjacent stimuli when they were labelled in the same way (both A or both B), and when one was labelled A and the other B; the respective means are 1·07 and 1·12. This eliminates the possibility that the experimental results could have been due not to the predicted effects of the classification but to some inherent differences in the judgements of the same stimulus when labelled A or B.

The following conclusions can therefore be drawn from the experimental results:

(i) A classification superimposed on a series of stimuli in such a way that there is a consistent and direct relationship between the magnitudes of the stimuli and the division of the stimuli into two classes determines a significant increase of the apparent differences between the stimuli at the point of transition from one class to another. This does not happen when the classification superimposed on the series does not stand in a coherent relationship to the physical dimension.

(ii) There is no direct evidence that this relationship between a classification and the judged physical dimension determines an increase in the judged similarity of the stimuli belonging to the same class.

(iii) There is no evidence that an increase in the extent of salience of the classification, as introduced in the present experiments, determined any accentuation of the effects of the classification. This negative finding cannot, however, be generalized. It is quite likely that the method used for enhancing salience was not adequate for its purpose; there is therefore the possibility that the salience of a classification may have some effect if more drastic methods are used to increase the relevance of the classification to the subjects' judgements.

(iv) There is no evidence that a repeated experience of the same classification after an interval of a week increases its effect on judgement.

(v) The evidence is very clear that the classification increases its predicted effects on judgement as a function of repeated experience during the same experimental session. This seems due to two judgement trends happening concurrently: the apparent inter-class differences in the classified series do not follow the trend of the control groups towards a progressive decrease in the apparent differences between the stimuli in the middle of the series; and there is a definite increase in the judged similarity of stimuli belonging to the same class. It is therefore possible that repeated and frequent experience of the same type of classification would lead, despite negative results reported in (iv) above, to an accentuation of its effects on judgement which would include both a relative increase in the subjective differences between the classes and in the subjective similarity within the classes.

These findings may possibly have some fairly wide implications for a variety of judgement situations. They represent, in a sense, a simplified exercise in stereotyping. An essential feature of stereotyping is that of exaggerating *some* differences between groups classified in a certain way, and of minimizing the same differences within such groups. It may be important to note that these effects were shown to exist in the present experiments despite the relative ease and simplicity of judgements, the minimal amount of experience with the classification, and its minimal significance to the subject. The drastic effects of a small but consistent and direct increase in the amount of experience with the classification can be seen when one considers the results of the additional trials in one of the experiments.

There is therefore the possibility that the phenomenon of stereotyping, occurring in situations where judgements are usually neither easy nor simple and where classifications have been built through long and continuously repeated past experience is no more than an exaggeration of the effects found in the present experiments. Several writers (e.g. Berkowitz, 1960; Campbell, Hunt & Lewis, 1957; Helson, 1959; Ittelson & Slack 1958; Sherif & Hovland, 1961; Rommetveit, 1960; Tajfel, 1959*a*) have recently attempted to show that many judgement phenomena found to occur in complex social situations can perhaps be reduced to relatively simpler principles found to be valid in psycho-physical judgement situations. These attempts have not met with general unqualified approval. For example, the following statement can be found in a recent book review: 'This underlies the point made by another reviewer that while it is quite legitimate to look for lawful relations in social psychology, social perception should not be treated as a kind of psycho-physics of social objects, but in terms of individual differences' (Moray, 1962, p. 58). One of the aims of the investigations reported here was to show that evidence for the essential unity of judgement phenomena, social or physical, can be slowly accumulated and that, without denying the importance of individual differences, it is possible to attempt an understanding of seemingly varied phenomena in terms of the same general judgement principles.

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EFFECT OF VISUAL DEPRIVATION ON AUDITORY VIGILANCE

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Signal detection in an auditory vigilance task is compared under conditions of normal visual stimulation and of visual deprivation produced by a blindfold. The task consists of detecting sequences of three different digits in the order odd-even-odd, appearing in a continuous series of digits. Sighted males detect significantly fewer signals than blindfolded males. The visual deprivation variable has no significant effect on female performance. There is some evidence from retrospective reports that sighted males are more emotional and perhaps aroused to a point which impairs efficient vigilance performance.

INTRODUCTION

The importance of variability in environmental stimulation for behavioural efficiency is emphasized in arousal theory (Hebb, 1955). Hebb has asserted that 'the normal variety of sensory stimulation has the function of maintaining arousal: the alertness, responsiveness, or "vigilance" of the waking subject'. Deese (1955), applying the arousal notion to vigilance, views the problem of maintaining vigilance as a problem in the maintenance of a background sensory input to offset the dissipation of an initial excitatory state of vigilance.

McGrath (1960) has tested a specific prediction based on arousal theory, namely that watchstanders will detect more signals when a variety of stimulation is present in the environment than in the absence of such variety, even though the increased variability has no relevance for the vigilance task being performed. This hypothesis is supported in two experiments. In one, increase in variability of auditory stimulation produced improvement in a visual vigilance task requiring detection of brightness increment of an intermittent light. In the other an increase in variability of visual stimulation produced improvement in an auditory vigilance task requiring the detection of loudness increments of an intermittent pure tone.

The present study constitutes an additional test of the arousal hypothesis in an auditory vigilance task. The procedure varies considerably from that used by McGrath. McGrath enriched normal visual stimulation by having the subject look at pictures. In the present experiment visual stimulation is manipulated in the opposite direction by eliminating it. A comparison is made between auditory vigilance performance with and without visual stimulation. According to a simple interpretation of arousal theory, visual deprivation, by reducing afferent stimulus variability, should result in decreased arousal and hence poorer performance in an auditory vigilance task.

An important difference between this study and that of McGrath is in the nature of the vigilance task employed. McGrath used a task which involved simple sensory detection, whereas the task used in this experiment may be said to be more cognitive, since it required the subject to listen to a series of digits for a defined signal. Thus it involved a short-term sequential memory factor as well as discrimination between even and odd digits. These differences in the task and in the method used to manipulate stimulus variability allow for a test of the situational generality of the arousal hypothesis. The design of the experiment also allowed for a study of the sex variable, as half of the subjects were male and half were female.

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* This paper is based on an M.A. thesis written by the junior author; it was written in its present form by the senior author while he was a visiting worker at the M.R.C. Applied Psychology Research Unit, Cambridge, England.

METHOD

Experimental design. A sample of 88 undergraduates, 44 male and 44 female, was used. Subjects were randomly assigned within sexes to a sighted or a visually deprived group. This constitutes a 2 × 2 factorial design with sex and visual deprivation as the main variables.

Vigilance task. An auditory vigilance task (Bakan, 1959) was used. Stimulus material consisted of apparently random tape-recorded digits presented at the rate of one per second for 48 min. Subjects listened for signals defined as *three successive different digits in the order odd-even-odd.* For scoring purposes the continuous task could be divided into three 16 min. periods, each containing six signals; time between signals within periods was 107, 71, 154, 25, 185 and 418 sec.

Practice. Prior to testing there were three practice periods. Immediately after reading instructions about the task the subject was presented with a mimeographed sheet containing 63 digits with instructions to search these visually for signals. There were seven signals on this sheet. Subjects received knowledge of results at the end of this session. There followed a 5 min. auditory practice period with stimulus material similar to that of the actual task. This contained two signals, and was followed by a 10 min. practice period containing three signals. In the auditory practice periods, knowledge of results was given by the experimenter calling out the digits of each signal 5 sec. after its appearance. A 5 min. rest followed before the start of the auditory vigil.

Test conditions. Subjects were tested in groups of two to six. Each subject sat in a cubicle visually isolated from others. A chair, a work table on which were placed a pack of response cards, a box to put them in and a pencil were in the cubicle. A tape recorder was located in a passageway between rows of cubicles; it was set at a volume easily audible to all subjects. Upon hearing a signal the subject wrote the digits constituting it on a 3 × 2½ in. card and placed it in the box. Sighted subjects put the card into the box through a thin slit at the top; visually deprived subjects did not use a slit because it was felt that finding a slit would impose an extra burden on them.

The visually deprived subjects wore goggles with gauze-covered rubber pads which exerted sufficient pressure on the eyelids to keep the eyes closed. During any particular test session all subjects were tested in the same condition. Subjects were not allowed to smoke, chew gum, or look at watches during the task. The visual environment of the sighted subjects was not enriched in any way.

Retrospective reports. Immediately after the vigil the subject was asked to respond to a list of 50 adjectives by indicating on a 7-point scale the degree to which each adjective described his feeling during the task. The adjectives used were selected for their relevance to states of arousal, motivation and emotion.

RESULTS

Signal detection. The basic data in terms of mean signal-detection for each of the four groups are shown in Fig. 1. The result of an analysis of the data (after square root transformation) is shown in Table 1.

The between periods variance, which reflects the performance decrement taking place between the first two periods and the third period, is significant at the 1 % level. None of the interactions involving periods is significant.

The overall difference between the sighted and the visually deprived subjects is not significant. However, the interaction between visual deprivation and sex is significant at the 5 % level. The significance of the sex variance must be considered in the light of this significant interaction.

The nature of the interaction between sex and visual deprivation is clear from inspection of Fig. 1. Here it is seen that the sighted male group is inferior to the other groups. The sighted male group is characterized by an earlier and larger decrement than the other groups. The overall difference in signal detection between the sighted and visually deprived male groups is significant at the 2 % level. There is no significant difference between the sighted and visually deprived female groups, and the small difference that there is favours the sighted group, this being a trend opposite in direction to that found for the males.

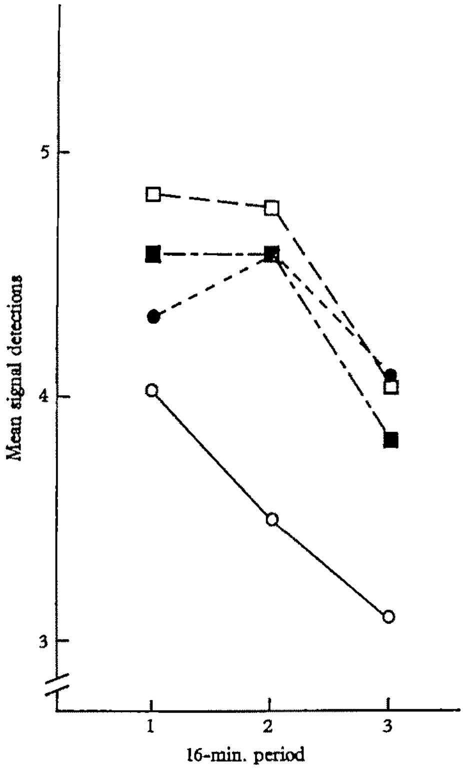

Fig. 1. Mean signals detected over time for males and females with and without visual deprivation. ○, Male sight; ●, male blindfold; □, female sight; ■, female blindfold.

Table 1. *Analysis of variance of signal detection data*

Source	D.F.	M	F
Visual deprivation (D)	1	0·57	2·78
Sex (S)	1	1·14	5·56*
D × S	1	1·28	6·24*
Subjects in same group	84	0·205	—
Periods (P)	2	0·890	6·10**
P × D	2	0·080	0·55
P × S	2	0·015	0·10
P × D × S	2	0·115	0·79
Residual	168	0·176	—
Total	262	—	—

* $P < 0{\cdot}05$; ** $P < 0{\cdot}01$.

False positive responses. A false positive constitutes the recording of a signal by a subject when in fact there was no signal. A signal was classified as a false positive if it (*a*) was properly definable as a signal, i.e. an odd-even-odd sequence; (*b*) was not a signal with two out of three correct digits, or (*c*) was not a signal correct except for transposition of digits, e.g. 541 for 145. The mean number of false positive responses was 2·8 for sighted subjects and 4·2 for visually deprived subjects; this difference does not approach statistical significance.

Retrospective reports. An analysis of the difference in the retrospective reports between sighted and visually deprived male subjects was carried out to see whether these reports might indicate some correlates of the difference between the groups in signal detection.

The overall trend of differences between the groups in retrospective reports tends to support the finding of differences in signal detection. The visually deprived males give a picture of themselves as efficient and positively motivated. Thus they report themselves on average as more *industrious*, *alert*, *wakeful*, *vigorous*, *alive*, *intent*, *co-operative*, *enthusiastic*, *calm* and *relaxed*. The sighted males present a picture of low efficiency and negative affect. They report themselves on average as more *tired*, *fatigued*, *restless*, *apathetic*, *dreamy*, *gone*, *dull*, *passive*, *careless*, *bored*, *upset*, *emotional*, *nervous*, *dismal*, *angry* and *excited*. Though most of the differences for particular adjectives are not significant, a pattern of differences does emerge.

An analysis of the difference between the groups in mean ratings produced four adjectives which discriminated significantly (5 % level) between the groups. These adjectives are *bored*, *dreamy*, *gone*, and *blind*. The difference for *blind* was the largest and clearly reflects the effect of the blindfold.

DISCUSSION

The main finding of this investigation is the superior performance of visually deprived males in an auditory vigilance task. Visual deprivation has no significant effect on the performance of females. It is interesting to note in this regard that there is some evidence of better vigilance performance for females (Wittenburg, Ross & Andrews, 1956).

A possible explanation of this result is that males are more easily distracted by irrelevant afferent stimulation. The reduction of visual stimulation eliminates a source of distracting stimuli, and performance is therefore improved. This explanation, though it has the attraction of simplicity, may need to be qualified. It has been shown by McGrath (1960) that exposure to irrelevant visual stimulation in the form of pictures to look at can improve performance in an auditory vigilance task. This is explained as a facilitating effect of an increase in arousal. The vigilance task used by McGrath involved a sensory discrimination whereas the task used in the present study required a more cognitive discrimination as well as a memory factor. In view of these differences the tasks may not be comparable. Another difference is that in the present study no attempt was made to structure the visual stimulation for the subject.

The present results may nevertheless be interpreted within the context of arousal theory. According to arousal theory (Hebb, 1955) performance suffers when arousal is either below or above an optimal level. There is no reason to believe that the sighted subjects were under-aroused as compared with the blindfolded subjects. If the difference between the two male groups is assumed to be real, a possibility that may be considered within the context of arousal theory is that the poorer performance of the sighted males was due to over-arousal.

There is some evidence in the retrospective reports suggesting that this explanation is plausible. On the basis of these reports it may be inferred that sighted subjects were more aroused than the blindfolded subjects. On words describing affect, especially negative affect, the sighted subjects tend to score higher. Thus the sighted group describes itself on average as more *bored*, *upset*, *emotional*, *fatigued*, *nervous*, *dismal*, *angry*, *excited*, and *restless*. In contrast the blindfolded group describes itself on average as more *industrious*, *enthusiastic*, *calm*, *relaxed*, *intent* and *co-operative*. Though differences on particular adjectives are not significant there appears to be a pattern suggestive of a difference between the groups in emotional tone; the sighted group appears to be more emotional and for this reason possibly at a level of arousal above the optimal level for efficient performance. It is not suggested that the increase in afferent stimulation in the sighted group is in itself sufficient to produce over-arousal, but rather that there are factors in the sighted condition that induce an emotional state in the male sighted subjects and it is this internal stimulation of emotional origin which leads to a state of over-arousal.

To complete the chain of reasoning it is necessary to speculate on the manner in which the presence or absence of visual stimulation can affect the emotional state of the subjects. It is suggested that the availability of a source of distraction is conducive to a conflict between attending to the relevant stimulation and attending to the irrelevant stimulation. This conflict results in the emotional state. The visually deprived subject with less distraction is less subject to the conflict and the subsequent emotional state, and is more likely to remain oriented to the auditory display. The fact that the blindfold procedure assures actual closure of the eyelids may also help to reduce the conflict since it is not habitual to see with the eyes closed. A comparison with a visual deprivation condition which allows the eyes to remain physically open would be of interest.

There is no evidence of the effect of visual deprivation on signal detection performance of the females. If the above reasoning is followed, this suggests that females are not susceptible to the conflict induced by the availability of visual stimulation. This lack of conflict may be related to the greater passivity of the females and possibly the greater likelihood of their following instructions.

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BEHAVIOURAL CONSISTENCY IN STRAINS OF RATS SELECTIVELY BRED FOR EMOTIONAL ELIMINATION

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An experiment is reported which confirms the difference in escape-avoidance conditioning between rats of the two Maudsley selected strains. This finding constitutes a second invariance in the behaviour of these strains: the first, with which it is contrasted, related to differences in conditioned emotional responses. It is suggested that the two nevertheless indicate a consistent emotional responsiveness in these strains.

I. Introduction

The Maudsley Reactive and Non-reactive Strains of rats, nos. 163f and 163g respectively in the *Catalogue of Uniform Strains* (1958) were established in 1954 by selectively breeding for high and low defecation in a revised and standardised version of Hall's open-field test (1934) of emotionality, and have now reached the 19th generation of selection (S_{19}). The procedures used and the psychogenetical results obtained have been described in detail elsewhere (Broadhurst, 1960, 1962). During this period they have been used as subjects in numerous other investigations, both behavioural and physiological, some of which have yielded evidence of inter-strain differences attributable to correlated responses to the genetical selection procedures employed. A series of investigations relating especially to one aspect of the physiological differences, thyroid function, is reported elsewhere (Feuer & Broadhurst, 1962*a*, *b*, *c*; Watson, 1960; *see also* Eayrs & Levine, 1963).

It is the purpose of this paper to report a further confirmation of one of the behavioural differences and to contrast it with another established difference found from the use of a different technique. These are escape-avoidance conditioning and the conditioned emotional response, respectively.

In establishing a conditioned emotional response or CER (Estes & Skinner, 1941), Singh used rats from three successive generations, S_6, S_7 and S_8, in three separate experiments (1959, 1961; Singh & Eysenck, 1960). The same apparatus was used in each case and consisted of a Skinner box in which the subjects were trained to a criterion of pressing a bar for a water reward. Fear conditioning followed in another box in which a flashing light was paired with a painful shock to the feet. The subsequent effect of the presentation of this light alone on lever pressing in the Skinner box was measured, and the response decrement so caused defines the CER. In the first experiment Singh found, *inter alia*, that there was a highly significant difference between subjects from the two strains: the reactive (high defecating) strain showed a greater response decrement than did the non-reactive (low defecating) strain, and this finding was confirmed in the two subsequent experiments. It should perhaps be mentioned that in these two cases this difference was complicated by an interaction with effects due to the drugs used as other experimental variables. Nevertheless, the overall effect was quite clear and was in the direction expected. It was interpreted as a greater susceptibility among rats of the emotionally reactive strain to conditioning of the fearful components of behaviour—crouching, urinating and defecating —which interfere with the bar pressing.

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The strains have also been used in a series of other experiments the results of which supported the notion that the reactive strain could be considered the more generally emotional (Broadhurst, 1957, 1958; Savage & Eysenck, 1963). In one test, however, escape-avoidance conditioning, the strain difference detected was in the reverse direction from that expected at the time. This finding was reported by Owen (1962) in a study in which rats of the S_{11} generation were required to learn to pass from one side of a shuttle box to the other in order to escape an electric shock to the feet, and then to avoid the shock by responding to an associated signal (a buzzer) which preceded it by 5 sec. Owen found the rats from the *non-reactive* strain learned the response more efficiently as measured by the number of trials needed to extinguish the avoidance habit. This finding was confirmed by Levine & Broadhurst (1963), who also found the non-reactives significantly better at avoidance learning, both in terms of the number of avoidance responses made and of trial latencies—the speed of responding to the signal. They used S_{14} animals in an improved apparatus in a complex experiment in which early experience was varied, and the present experiment was, therefore, undertaken to verify this confirmation using the same improved apparatus and a simpler experimental design. It was decided to vary the shock intensity used in order to see if increasing it would affect the relative standing of the two strains.

II. METHOD

(i) *Subjects*

Sixty rats from the S_{17} generation, among which both strains and sexes were equally represented, were randomly assigned to three different shock intensities, 0·2, 0·5 and 0·8 mA. Thus we have a 2 × 2 × 3 design with 5 replications. The subjects were 163·1 ± 6·1 (S.D.) days of age when given the conditioning test. They were not previously tested in the open field: the previous experiment having shown that the strains are now so well separated in their scores in this test (Broadhurst, 1962) that subjects need not be measured individually. Moreover there were some indications of a difference between the strains in the order effects of the two tests—open field and conditioning—and it was desired to avoid this complication.

(ii) *Apparatus and procedure*

Conditioning was accomplished in a single session in which 50 trials were given at irregular intervals (average 50 sec.) in an automatic apparatus described elsewhere (Levine & Broadhurst, 1963). Briefly, it may be said that, in addition to the three levels of shock used for the unconditioned stimulus, as noted above, the conditioned stimulus, a buzzer, preceded the onset of shock by 8 sec. An escape response by the rat terminated both the shock and the buzzer; an avoidance response terminated the buzzer before the shock onset and is consequently so defined.

Programming of these stimuli and recording of the responses and their latencies was automatic.

III. RESULTS

Analysis of variance of the average avoidance (only) latencies, after a square root transformation had removed the inhomogeneity of variance, showed a single, highly significant effect. This was the strain difference and it reached the 0·1 % level, the reactive strain having a mean time of 2·0 ± 0·3 sec. and the non-reactive, 1·3 ± 0·2 sec.

The avoidance data did not yield to transformation but a preliminary analysis of variance nevertheless performed indicated a similar highly significant strain difference, the reactive strain having a mean of 23·7 ± 12·2 avoidances as compared with 39·2 ± 6·0 for the non-reactives. This general finding encouraged us to analyse the data for the two strains separately, both sets now showing satisfactory homogeneity. The analysis of variance of the reactive strain data yielded no significant effects, but that for the non-reactive strain showed that the main effect for shock level approached the 5 % level of significance, the tendency—evident throughout these data—being for the stronger shock to evoke *fewer* avoidance responses.

IV. DISCUSSION

These findings confirm those from the previous experiment using the same apparatus (Levine & Broadhurst, 1963). Once again the reactive strain has been shown to be slower than the non-reactive both in acquiring the avoidance response and in executing it, and the difference reached the same high level of significance. The actual values for the two measures of course vary owing to the difference in the experimental designs, especially the use of a single shock level (0·25 mA.) previously. In addition the earlier experiment yielded a significant sex difference in avoidance scores confined to the reactive strain and in the direction of more responses by females. But the absence of any sex differences in latencies is found in both experiments.

Thus the superiority of the non-reactive strain in avoidance learning can now be regarded as a well-established feature of the behavioural repertoire of the Maudsley strains. It contrasts with the well-established superiority of the reactive strain in the formation of conditioned emotional responses. To what should this contrast be attributed? The first possibility is that the strains have changed in the three generations intervening between Singh's last experiment (Singh & Eysenck, 1960) and Owen's (1962). This possibility cannot be excluded in the absence of an experiment using the same subjects in both situations, but it is rendered unlikely by our experience of another invariance in the differential behaviour of the two strains. This is their reaction to air deprivation as measured by the speed of underwater swimming to escape. While not so well established by several experiments on successive generations as the inter-strain differences at present under discussion, the significantly greater swimming speed of the reactive strain was first detected (Broadhurst, 1957, 1958) in rats of S_2 and S_3 and was later confirmed among rats of S_{13} (Broadhurst, 1963). Now this difference in time is several times greater than that spanned by the present two series of experiments and yet no reversal of the strains' emotional responsiveness is suggested; on the contrary yet another stability is implied. These considerations lead us to discount the possibility that the strains have themselves reversed behavioural roles and to seek an explanation elsewhere.

In our view, the solution lies in the suggestion (Denniston, 1959; Owen, 1962; Savage & Eysenck, 1963) that the interference of responses to the shock stimulus which are incompatible with efficient avoidance responses, such as freezing, are implicated. Now the conditioned emotional response was elaborated in a situation apart from the one in which its effect, as a competing response to the bar pressing habit, was observed. Hence we had a situation in which the postulated greater susceptibility of the reactive strain to such emotional responses—as evidenced by their open-field test scores—would be capitalized upon since it enhances the decrement in the behavioural response measured. But in the conditioning test, on the other hand, the situation is reversed. Emotionality would tend to lead to inactivity which impedes the motor responses required, and hence the reactives would be at a disadvantage.

In this way the behavioural characteristics which have been developed in the Maudsley strains by the selection practised appear to manifest themselves quite consistently. The situations in which they are displayed happen to have been arranged in such a way that in the one case, the CER, the response to the shock stimulus used is seen as an apparent increase of emotional responsiveness, whereas in the other, escape-avoidance conditioning, it appears as a lowering of responsiveness.

However, the nature of the mechanism involved is not as yet clear. One possibility lies in the suggestion in the present data that the higher shock levels occasioned less avoidance learning and slower speed. That is, the greater the intensity of fear arousal, the poorer the conditioning. This doubtless occurs after some optimum point (Broadhurst, 1959) in the scale of shock intensity which we have not yet sampled, being below the intensities we used. Now, if we suppose that the reactive strain rats enter the conditioning situation with a greater degree of constitutional fear arousal than the non-reactive ones, then this might be functionally equivalent to a higher shock intensity. However, further work will be needed to test this and the other possible explanatory hypotheses which have been advanced (Owen, 1961).

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THE EFFECT OF IRRELEVANT CUES ON REVERSAL LEARNING IN THE RAT

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Twenty-four rats were trained on a brightness discrimination, given 0 or 150 overtraining trials, and then reversed. For one group only one irrelevant cue (position) was present throughout the experiment. For the other two groups the reversal was learnt with orientation as a second irrelevant cue; in one case this cue had been present during original learning, in the other it had not. Where there were two irrelevant cues, overtraining was shown to have a greater facilitating effect on reversal than where there was only one—whether or not the second cue had been present in original learning.

The results are interpreted as suggesting that the effect of overtraining is to increase the probability that responses will be controlled during reversal by the relevant stimulus dimension.

I. Introduction

It has been shown for octopuses (Mackintosh & Mackintosh, 1963) that overtraining has a significant facilitating effect on reversal learning only where the discrimination is one involving irrelevant cues. No effect of overtraining was found in a simple successive training situation, while in a simultaneous training situation and in a successive situation where the positive and negative stimuli were randomly either horizontal or vertical rectangles (i.e. with position and orientation respectively as irrelevant cues) overtrained groups learnt the reversal significantly faster than non-overtrained groups.

From this one would expect that in a simultaneous situation the addition of an extra irrelevant cue should increase the effect of overtraining. Part of the present experiment was designed to test this prediction for rats: two groups of animals were trained in a Lashley stand on a brightness discrimination and its reversal (with or without overtraining). For one group the stimuli were black and white squares; for the second group the stimuli were black and white rectangles of the same area as the squares, one of which on any trial was horizontal and the other vertical. The positive and negative were each equally often horizontal and vertical, thus the orientation of the rectangles was irrelevant to the successful solution of the problem. It was predicted that the effect of overtraining would be greater for the second group than for the first.

Such a result might be anticipated on more than one theory of reversal learning. Harlow (1959), for example, has suggested that the effect of overtraining is to eliminate Error Factors, i.e. tendencies to respond systematically to incorrect aspects of the problem—among others, clearly, irrelevant cues as here defined. An alternative explanation of reversal learning suggested by Sutherland (1959) has argued that the effect of overtraining is to increase the likelihood that responses will be controlled by the relevant stimulus dimension. This is achieved by proposing a two-stage model of discrimination learning. First an analyser specific to the relevant stimulus dimension is switched in, and secondly approach and avoidance responses are attached to the outputs of the analyser representing the positive and negative stimuli. Overtraining is assumed to have the effect of strengthening the first process so that the analyser remains switched in after the overt choice responses have been extinguished, thus ensuring that the problem will still be solved in terms of the relevant dimension.

On both these accounts overtrained subjects should be less likely to respond to irrelevant cues upon reversal and thus learn the reversal faster; in Harlow's case this is because such tendencies have been adapted out as a result of training with that dimension present, in Sutherland's case because the correct analyser has been firmly switched in. Thus Sutherland would predict a decreased tendency to respond to an irrelevant cue upon reversal, even if it was not present during original training; while Harlow would predict such an effect only where the cue was present from the beginning of training. To test this, a third group of rats was run, trained originally to discriminate between the black and white squares, and upon reversal between the black and white horizontal or vertical rectangles.

For this last group reversal is distinguished from original learning not only by the fact that opposite brightnesses are now rewarded, but also because the stimuli are now of a different shape. Another possible explanation of faster reversal learning following overtraining is that overtrained animals extinguish faster to their original positive stimulus (Capaldi & Stevenson, 1957; Birch, Ison & Sperling, 1960). Such a prediction might be derived from the 'Discrimination Hypothesis' of extinction. Since reversal and training situations are especially discriminable for the group trained in the way described above, it would be predicted that the overtrained animals in this group will extinguish even more rapidly. Accordingly, as a measure of extinction, records were taken of the tendency to respond to the original positive stimulus during early reversal trials.

II. METHOD

Subjects

The subjects were 24 experimentally naïve male hooded rats from the colony maintained at the Institute of Experimental Psychology, Oxford. They were 3 months old at the beginning of the experiment.

Apparatus

A modified Lashley jumping stand was used. The two stimulus windows (14 cm. square) were set at an angle of 130°, and separated by a partition 35 cm. high and 11 cm. deep. The jumping platform was Y-shaped, so that each arm faced one of the stimulus windows; its level was 3 cm. above the bottom of the windows. Landing platforms 15 cm. long and 9 cm. deep were fixed below each window. The gap between jumping and landing platforms could be varied: at the beginning of pretraining it was 5 cm., during training 16 cm. The feeding platform behind the windows could be divided into two by a movable partition and contained two round feeding bowls. The whole apparatus was painted flat mid-grey, and was illuminated by a 40 W. lamp placed 20 cm. behind and 20 cm. above the jumping platform and directed on to the windows.

The stimulus doors were made of brown hardboard 15 cm. square and 0·3 cm. thick, and were supported on a ledge behind the windows. The stimuli were cut from $\frac{1}{8}$ in. opaque perspex sheet, and glued centrally to the doors. One set of stimuli consisted of 6·5 cm. black or white squares, the other set of 10·5 × 4 cm. black or white rectangles.

Procedure

Feeding schedule. Seven days before pretraining began, subjects were put on a feeding schedule, beginning with 6 hr. food per day, and gradually reducing to 2 hr. food per day. Water was available in the home cages at all times.

Pretraining. Seven days of pretraining were given. For the first 3 days subjects received their entire daily ration of food on the apparatus. They were fed in groups of four for 1½ hr. The partition dividing the feeding platform was removed, and the jumping platform was 5 cm. from the landing platforms. Subjects were thus allowed to explore the whole apparatus. On the next 2 days subjects were again fed in groups of four on the apparatus for 1½ hr. In addition, four trials were given each day, training subjects to jump through the open windows. The jumping platform was at first close, and gradually moved back to the full 16 cm. Manual guidance was given where necessary to ensure an equal number of responses to each side. On the last 2 days subjects were given a further 6 trials per day with blank hardboard doors fitted, again with manual guidance where necessary. Between trials they were kept in individual detention cages used later during training. They were then returned to their home cages and fed for 1½ hr.

Training. Subjects were run 10 trials a day by the non-correction method. If they jumped to the correct door, they could push it open and run through to the food bowl behind, where they were allowed to eat for 20 sec. After an incorrect jump, they found the door locked and were left on the landing platform for 10 sec. No punishment was given for incorrect responses. They were fed for 1½ hr. in their home cages after their daily trials. Inter-trial interval was 5–7 min. The criterion of learning used was 18 correct responses out of 20 consecutive trials, with the last 10 correct. The stimulus doors were changed from side to side according to selected random orders. For Group II during original learning, and for both Groups II and III during reversal, a second random order determined whether the positive stimulus was the horizontal or the vertical rectangle (the negative was automatically the other rectangle). Within each day's trials, the positive was equally often horizontal and vertical, and over 2 days' trials, each of the positive rectangles appeared equally often on each side.

Table 1. *Mean scores during original learning and reversal*

			Reversal learning				
					Days with responses to		
Group	No. of overtraining trials	Original learning. Days to criterion	Days to criterion	Days to extinction	Position	Orientation	Days to re-acquisition
I	0	8·35	14·17	5·00	7·25	—	9·17
	150	8·25	12·62	6·00	4·75	—	6·62
II	0	10·02	20·30	3·75	14·00	0·5	16·55
	150	10·10	14·07	7·25	5·25	0	6·82
III	0	8·57	19·35	3·50	12·00	1·5	15·85
	150	8·22	13·32	7·00	4·75	0	6·32

Experimental design

There were three main experimental groups, each of eight subjects. Within each group four subjects were given 150 overtraining trials, four no overtraining, and half of each of these subgroups was trained with black, half with white positive.

Group I was trained and reversed on the black and white squares.

Group II was trained and reversed on the black and white rectangles.

Group III was trained on the squares and reversed on the rectangles.

The assignment of subjects to the three main groups and to black or white positive was random, but to overtrained or non-overtrained subgroups on the basis of original learning scores in such a way as to equalize these scores for each pair of subgroups.

III. RESULTS

The results are presented in Table 1. For original learning only one measure is given: number of days to reach criterion. For reversal learning five measures are given: total number of days to reach criterion; and in addition the number of days on which the majority of responses were made to the original positive stimulus, or to one or other position, or (for Groups II and III) to one or other orientation. The first was used as a measure of extinction of approach tendencies to the original positive, the second and third as measures of tendencies to respond to the irrelevant cues. The final measure shown in Table 1, labelled re-acquisition, was arrived at by subtracting the number of days on which the majority of responses were to the original positive from the total number of days to reach the criterion of learning. The results of subjects trained in opposite directions are for the sake of simplicity combined, although on some measures subjects learning with black positive were superior to subjects learning with white positive.

Original learning

An analysis of variance was performed on original learning scores, but there were no significant differences between groups (for all main effects, $F < 1$). As can be seen, overtrained and non-overtrained subgroups were closely matched.

Reversal learning

Analyses of variance were performed on all scores shown in Table 1. As stated in the Introduction, both Harlow and Sutherland would predict a greater effect of overtraining for Group II than for Group I, but while Sutherland would predict no difference between Groups II and III, on Harlow's theory overtrained subjects in Group III should learn the reversal more slowly than those in Group II. As can be seen from Table 1, the scores for both overtrained and non-overtrained subjects in Groups II and III were very similar, and such differences as there were favoured Group III rather than Group II. Accordingly in all analyses of the group effect (except those for the number of extinction days and for responses to orientation), the scores of subjects in Groups II and III were treated together.

Days to criterion

The three main effects were all significant: that between Group I and Groups II and III at the 0·05 level ($F = 7{\cdot}8$, D.F. 1, 19); that between overtrained and non-overtrained subjects at the 0·001 level ($F = 16{\cdot}4$, D.F. 1, 19); and that between subjects reversed to black positive and subjects reversed to white positive at the 0·001 level ($F = 19{\cdot}3$, D.F. 1, 19). The difference between Group I and Groups II and III was almost entirely due to the non-overtrained subgroups. Analyses of variance of overtrained and non-overtrained subgroups separately showed that although there was a significant difference for non-overtrained subjects between main experimental groups ($F = 17{\cdot}0$, D.F. 1, 11, $P < 0{\cdot}01$), the difference for overtrained subjects was not significant ($F = 1{\cdot}5$, D.F. 1, 11, $P > 0{\cdot}10$). In the main analysis this showed up as an interaction between experimental groups and overtraining which was significant at the 0·05 level ($F = 4{\cdot}4$, D.F. 1, 19). In summary, overtraining had a significant effect on subsequent reversal learning; this effect was greater for Groups II and III than for Group I, since although overtrained subjects within all three groups took approximately the same number of days to learn, the non-overtrained subjects in Groups II and III took longer than those in Group I.

Extinction

There was no significant difference between groups ($F < 1$); but overtraining and positive stimulus during reversal both had significant effects (in both cases $F = 8{\cdot}0$, D.F. 1, 15, $P < 0{\cdot}05$). No interactions were significant. It is important to notice that Group III did not differ on this measure from the other two groups, nor were the overtrained subjects in this group especially fast in ceasing to respond to their original positive. In fact all overtrained subgroups responded for a longer time than all non-overtrained subgroups.

Responses to position

The group effect was significant at the 0·05 level ($F = 4{\cdot}6$, D.F. 1, 19); overtraining at the 0·001 level ($F = 21{\cdot}9$, D.F. 1, 19); and positive stimulus during reversal at the 0·05 level ($F = 4{\cdot}7$, D.F. 1, 19). The interaction between groups and overtraining just failed to reach significance ($F = 4{\cdot}1$, D.F. 1, 19, $0{\cdot}10 > P > 0{\cdot}05$). Group I made fewer position responses than Groups II and III; from Table 1 it can be seen that this is due to the non-overtrained subjects.

Responses to orientation

The group effect (Group II against Group III) was significant at the 0·05 level ($F = 4{\cdot}8$, D.F. 1, 12); overtraining was significant at the 0·001 level ($F = 19{\cdot}2$, D.F. 1, 12); positive stimulus had no effect ($F = 2{\cdot}4$). Inspection of Table 1 shows that only Group III_0 made many responses to orientation, and that all overtrained subjects had a score of 0 on this measure.

Re-acquisition

With one exception the results using this measure are the same as those from overall days to criterion. They are included because it is arguable that this is a more accurate measure of the acquisition of the reversal. The exception is that the effect of positive stimulus during reversal is not quite significant ($F = 3{\cdot}5$, D.F. 1, 19, $0{\cdot}10 > P > 0{\cdot}05$). Thus the faster overall learning in subjects with black positive is due more to faster extinction than to faster re-acquisition. The group effect is significant at the 0·05 level ($F = 7{\cdot}1$, D.F. 1, 19); overtraining at the 0·001 level ($F = 33{\cdot}8$, D.F. 1, 19), and the interaction between groups and overtraining at the 0·05 level ($F = 7{\cdot}5$, D.F. 1, 19). From Table 1 it can be seen that the three overtrained subgroups have virtually identical scores on this measure; the slight differences between their overall reversal scores are mostly due to different rates of extinction.

IV. Discussion

The experiment showed that the facilitating effect of overtraining on reversal learning was greater where there were two irrelevant cues in the reversal situation than where there was only one such cue—whether or not the second irrelevant cue was present during original learning. Upon reversal, overtrained subjects responded longer to their original positive stimulus than non-overtrained subjects, even where the shape of the stimuli was altered between original and reversal learning.

Extinction scores

If it is accepted that the measure used here is valid, then these results would appear directly to contradict predictions from a theory of reversal learning in terms of faster extinction following overtraining. Furthermore, Group III_{150}, which on the 'Discrimination hypothesis' would be expected to distinguish training and reversal conditions faster than any other group, showed no such tendency, and continued for an average of seven days to respond predominantly to their original positive stimulus. Another measure of extinction taken in an earlier experiment (Mackintosh, 1962), and also given by Reid (1953)—namely number of initial consecutive errors, shows the same tendency for overtrained animals to extinguish more slowly than non-overtrained animals.

The effect of irrelevant cues

The results on the number of responses to position during reversal confirm earlier findings (Mackintosh, 1962; Mackintosh & Mackintosh, 1963). The course of learning for overtrained and non-overtrained subjects is different. The former extinguish more slowly but spend less time responding to this irrelevant cue than the latter. The first difference, as mentioned above, is directly contradictory to Capaldi & Stevenson's account, and the second seems to demand an entirely different set of assumptions; both, however, would be predicted on Sutherland's and Harlow's theories.

The presence of a second irrelevant cue (orientation) significantly increased the effect of overtraining. It is important to see how this effect appears. All three overtrained groups learnt the reversal at the same speed—thus reversal learning for overtrained animals is unaffected by irrelevant cues. It is the non-overtrained animals who show the effect—and are greatly hindered by irrelevant cues. Consequently the effect of overtraining is more marked for Groups II and III than for Group I. These results are closely parallel to those obtained with octopuses; thus although the number of animals involved in both experiments is small, the results are clearly reproducible. The additional finding of the present study is that an overtrained group which is not introduced to the irrelevant cue of orientation until after original training and overtraining is complete, is able to ignore this irrelevant cue during reversal just as well as a group trained originally with the cue present. This finding suggests that Harlow's Error Factor theory is insufficient to account for the results. Sutherland's theory, however, will handle all these points. Irrelevant cues are ignored by overtrained animals because they have learnt to switch in the appropriate analyser, and thus respond only in terms of the relevant cue; non-overtrained animals are hindered by irrelevant cues because they are likely to switch out the appropriate analyser and respond to any irrelevant cues present—the more there are, the more they will be hindered.

It is of course true that the problems presented to the three groups during reversal are different (although all are learning a brightness discrimination, for Group I the stimuli are squares, for Groups II and III rectangles). It might be argued that comparisons between groups trained on different problems in this way (comparisons upon which the above conclusions depend) are unjustified, especially as there is a suggestion that the rectangles discrimination is initially more difficult than the squares. However a number of points may be adduced against this argument. (1) The difference in problem difficulty between the squares and rectangles discriminations did not even approach significance during original training ($F < 1$) whereas (the important comparison) for non-overtrained subjects during reversal the difference was highly significant ($P < 0{\cdot}01$). (2) For overtrained subjects during reversal there was no difference at all between the two problems. Thus it is only during reversal, and then only for non-overtrained subjects, that the problems are of unequal difficulty. It is important to notice that even Group III_{150}, who had never previously seen the rectangles, learnt the reversal as fast as the other overtrained groups. Nor is it plausible to account for this by saying that this is merely because overtraining has (as it always does) reduced position habits. Overtraining does not necessarily do this: when the second problem to be learnt is in a different dimension from that employed during initial training, overtraining *increases* the strength of position habits, and hinders learning of the new problem (Mackintosh, 1962). (3) Finally, in the experiment with octopuses previously referred to, where precisely similar results were obtained, it was found that one of the problems with irrelevant cues was originally learnt with far *fewer* errors than the problem without irrelevant cues: in spite of this initial discrepancy in favour of the irrelevant cue problem, during reversal non-overtrained subjects made significantly more errors learning this problem than learning the problem without irrelevant cues.

In one respect the results are not entirely consistent with Sutherland's theory. It was stated in the Introduction that 'overtrained animals should be less likely to respond to irrelevant cues upon reversal'. For position this was confirmed for all groups; but for orientation only for Group III; in Group II only two subjects (both non-overtrained) scored one day's responses to orientation. In general the slow learning of non-overtrained subjects in Groups II and III clearly cannot be accounted for solely in terms of the number of days on which responses were predominantly to orientation;. it is due rather to an increase in the number of position responses. A possible explanation might be that although the subject does not attempt to solve the problem in terms of orientation, nevertheless the presence of this irrelevant cue makes the abstraction of the relevant brightness differences more difficult, and position responses are merely an index of this. A similar result has been reported by Lawrence & Mason (1955), who found that 'the introduction of additional irrelevant dimensions into a discrimination reversal situation results in a disproportionate increase in systematic runs to position' (*op. cit.* p. 7).

Finally it should be noted that these results cannot reasonably be explained by the concept of a 'response of discriminating' (Reid, 1953), if by this is meant an overt orienting response. Such a response could not be responsible for selecting brightness cues and ignoring orientation cues where both sets of cues are contained in one set of stimuli. Goodwin & Lawrence (1955) have also argued that their results cannot be explained by such a concept, and concluded that the selection of one cue rather than another must be made by some internal mechanism of 'attention'; their theory in fact is similar to Sutherland's. The results obtained here provide even better evidence, in that while the two sets of cues used by Goodwin & Lawrence were only spatially contiguous, here they were superimposed on the same figures. Furthermore, North & Clayton (1959) have shown that where relevant and irrelevant cues *can* be distinguished by an orienting response (i.e. where they are spatially separate) reversal learning is unaffected by the presence of irrelevant cues. This suggests that even without overtraining the orienting response is established sufficiently well not to be extinguished by reversal, and therefore that the effect of overtraining on reversal cannot be accounted for by supposing that overtraining strengthens it.

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PAIRED-ASSOCIATE LEARNING AS A FUNCTION OF STIMULUS AND RESPONSE SATIATION*

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Two experiments, one using highly meaningful pairs and the other using low meaningful pairs, were performed to determine the effects of stimulus and response satiation in paired-associate learning. Results indicated that: (*a*) satiation treatment of highly meaningful words produces a decrease in their meaning, whereas similar treatment of low meaningful words does not produce significant semantic change; (*b*) with highly meaningful S–R pairs, pre-learning satiation of either the stimulus or the response words retards subsequent learning, whereas with low meaningful S–R pairs only stimulus satiation produces such retardation. Results are explained in terms of two factors involved in the satiation procedure: a meaning decrement and the development of a word-word habit. The importance of these two factors for stimulus and response positions is discussed.

INTRODUCTION

Recent studies have demonstrated that the meanings of verbal symbols can be reduced in intensity if the symbols are continuously repeated aloud (Lambert & Jakobovits, 1960; Kanungo & Lambert, 1962). Subsequent examination of this phenomenon, referred to as semantic satiation, indicates that decreases in the meanings of symbols adversely affects the efficiency with which symbols are learned (Jakobovits & Lambert, 1962; Kanungo, Lambert & Mauer, 1962). The latter study has shown that the satiation treatment given to meaningful words before they are encountered as response members of a paired-associate list reduces the rate of learning of such lists. The present experiments were designed to reveal whether stimulus and response members of paired-associate lists were equally vulnerable to the satiation treatment.

Studies of the meaningfulness of stimulus and response members of paired-associate lists reveal that variations in the meaningfulness of responses produces a greater difference in the rate of learning than do corresponding variations in the meaningfulness of stimuli (Underwood & Schulz, 1960, pp. 39ff.). Cieutat, Stockwell & Noble (1958), for example, had found that 'at least twice the variance in performance due to meaningfulness (*m* or *m'*) is attributable to *R* as to *S*' (p. 201). In view of the greater importance of meaning for response members in paired-associate learning, it follows that a decrease in the meaning of response members should produce greater inhibitory effects on the acquisition of the paired-associates than should a similar decrease in the meaning of the stimulus members. In other words, pre-learning satiation of response members of highly meaningful paired-associates (H–H) should effect greater negative transfer than pre-learning satiation of stimulus members. If one assumes that the satiation treatment experimentally reduces a highly meaningful verbal unit (H) to a less meaningful one (L), it follows

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that paired-associates with both elements highly meaningful (H–H) would become more difficult to learn if the response members are satiated because the original H–H list would then appear as an H–L list. Similarly, if stimulus members are satiated, an H–H pair is reduced to an L–H state at the time of learning. Because any change in the meaning of the responses is more influential than similar changes in the stimuli, it would be expected that pairs would be more difficult to learn following either stimulus satiation (leading to L–H learning) or response satiation (leading to H–L learning) than would control pairs not given the satiation treatment, and more difficulty would be encountered after response than after stimulus satiation.

EXPERIMENT I

Subjects

Forty-eight English-speaking cadets of the Royal Canadian Air Force who were enrolled in a 6-week training course at a base near Montreal volunteered to be subjects for the experiment. Each subject was tested individually at the training base during regular work hours.

Material and apparatus

Three lists of paired-associates were prepared, each list containing 10 pairs of meaningful words. Both stimulus and response members of each list were chosen on the basis of their high frequency of usage (Thorndike & Lorge, 1944) or their high connotative meaning (Jenkins, Russell & Suci, 1958). The first list was used to equate groups of subjects while the second and the third lists constituted a measure of the dependent variable in the main stage of the experiment. The second list was the same as the third except that stimulus–response position of each pair in the second list was reversed in the third list. Each list was printed on a strip of paper in four different random orders, suitable for a standard anticipation procedure for presentation on a memory drum. A 3 sec. interval separated the presentation of the stimulus and the stimulus–response pair, and a similar interval elapsed before the presentation of the next stimulus. The inter-trial interval was 6 sec.

Ten other highly meaningful words were chosen as controls on the same basis as that described above. Care was taken that the control words were not semantically related to either the stimulus or the response words of the second or third lists. These words were used as controls in the sense that they were not to enter into the learning task after they had been given the satiation treatment.

Three semantic differential scales (good–bad, active–passive, strong–weak) were used to measure the intensity of the semantic ratings of the words included in second and third lists and as controls. Each of these words was printed on a separate 3 x 5 index card. Each semantic scale was also printed on a separate card. All cards were then placed in a Kardex folder so that they could be exposed in a predetermined random order one at a time, first a word and then a semantic scale along which the subject gave his rating of the word just presented.

Procedure

All forty-eight subjects were tested twice, At the start of the experiment, the subject learned the first list to a criterion of three consecutive successful anticipations. On the basis of learning scores, four groups, equated for both trials and errors, were formed and each group was assigned to a different condition in the main stage of the experiment. Two of the four groups served as controls, the Retest Control (RC) and the Different Word Satiation (DWS) groups. The other two served as experimental groups, the Stimulus Satiation (SS) and the Response Satiation (RS) groups. Furthermore, in each group, six subjects were given the second list to learn whereas the other six subjects were given the third list to learn. Care was taken to insure that the subgroups of subjects for each group were matched with respect to learning scores in the initial test. Since the second and third lists differed only in the stimulus–response position of particular words, any possible word-position effect was counterbalanced. This control was necessary because the items in both the stimulus and response positions of any pair were not perfectly matched for their meaningfulness. Reversing their positions for half of the subjects made the initial meaningfulness level of stimulus–response positions of the pairs equivalent for the group.

The main stage of the experiment started approximately one week after each subject's initial testing. For subjects of the RS group, the normal semantic profile was taken for each of the ten response words of the paired-associate list assigned them. Each word was exposed for 1 sec. and the subject was asked to indicate its appropriate semantic placement by pointing to one of the seven positions on a semantic scale. This was done for all words on all scales, a total of 30 responses per subject. The order of presentation was randomized to maximize the separation of the re-occurrence of each word and each scale. For the satiation treatment, each response word was again exposed for 1 sec. and the subject was asked to repeat the word aloud continuously for 15 sec. at a rate of 3–4 repetitions per sec. Immediately after the repetition, the subject made his rating for that word on a semantic scale. This procedure was also repeated three times for each of the ten response words, once for each semantic scale, and the order of presentation was also randomized as was done while taking the normal semantic profile. Subjects of the SS and DWS groups received exactly the same type of treatment as was given to the RS group except that the ten words which were satiated were stimulus words and control words respectively instead of response words. Immediately after the satiation treatment, each subject of the RS, SS, and DWS group was presented with the second or third list, whichever was designated for him to learn, and similarly each subject of the RC group, receiving no satiation treatment, was also presented with either the second or the third list to learn. The standard anticipation procedure was used and a learning criterion of three consecutive errorless trials was set.

Results and discussion

Analysis of variance performed on both the trials and error scores in the initial test for the four groups revealed that the attempt to equate all four groups on the basis of their initial learning measures had been successful.

Table 1 presents the results of an analysis of variance performed on the semantic ratings for the three groups of subjects who were measured both before and after the satiation treatment. An examination of Table 1 reveals that the value of F of only one main effect, differences in semantic ratings taken before and after satiation treatment, is significant at the 0·01 level. Since the mean semantic ratings for each group decreased after the satiation treatment, it can be safely concluded that satiation treatment applied to highly meaningful words promotes significant decrements in their meanings. Insignificant between-group comparisons and interactions mean that the groups were neither significantly different with respect to their initial ratings, nor were their final ratings differently affected by the satiation treatment.

Table 1. *Analysis of variance of semantic ratings for three groups (groups RS, SS, and DWS) before and after satiation treatment, Expt. I*

Sources of variation	Sums of squares	D.F.	Mean square	F
1. Between groups	7·092	2	3·546	0·719
Between subjects in the same group	162·753	33	4·931	—
Total between subjects	169·845	35	—	—
2. Between conditions (before and after satiation treatment)	4·108	1	4·108	11·072*
1 × 2	0·167	2	0·083	0·223
Pooled subjects × 2	12·275	33	0·371	—
Total within subjects	16·55	36	—	—
Total	186·395	71	—	—

* $P < 0{\cdot}01$.

To determine whether the experimental and the control groups differed in their rate of learning in the main test, a one-way analysis of variance was performed on both trial and error scores for the four groups. The results, presented in Table 2, reveal that the between-group F ratio for the trial measures is significant at the 0·03 level, but the between-group F ratio for the error measures only approaches the significance level ($P < 0·06$). Since there was a possibility that the learning measures of the initial test were positively correlated with the learning measures of the main test, a covariance analysis was performed to increase the precision of the significance test. Table 3 presents the results of the analysis of covariance. It is clear from Table 4 that with the initial learning ability equated, the significance levels of trial and error scores are raised to 0·005 and 0·025 respectively. Furthermore, the within-group correlations, which are 0·59 ($P < 0·01$) in the case of both trial and error measures, indicate that there is a substantial correlation between initial and main test learning scores under any given condition.

Table 2. *Analyses of variance for learning data of the main test, Expt. I*

		Trial measures			Error measures		
Sources of variation	D.F.	Mean square	F	P	Mean square	F	P
Between groups	3	74·743	—	—	1486·356	—	—
Within groups	44	21·547	3·468	< 0·03	544·793	2·728	< 0·06

Table 3. *Analyses of covariance of the performance of four groups of subjects Expt. I*

Learning measures	Sources of variation	Sums of squares	D.F.	Mean square	F	P
Trials	Adjusted means	239·119	3	79·706	5·531	< 0·005
	Within groups	616·619	43	14·409	—	—
Errors	Adjusted means	4565·687	3	1521·895	4·169	< 0·025
	Within groups	15696·635	43	365·038	—	—

Since the covariance analysis resulted in significant F values, adjusted criterion means for each group were computed. These mean learning scores for the main test are as follows:

Group	Mean no. of trials	Mean no. of errors
RS	15·06	41·30
SS	14·66	45·34
DWS	9·85	23·79
RC	11·18	36·00

To test the significance of the mean differences between any two groups, the critical differences of 3·13 for trials and 15·76 for errors to be significant at the 0·05 level; and the critical differences of 4·19 for trials and 21·09 for errors to be significant at the 0·01 level were computed. No significant difference could be observed either between the two control groups, DWS and RC, or between the two experimental groups, RS and SS. However, comparisons of each of the experimental groups with the control groups indicated that in the case of trial measures both DWS and RC groups showed significantly better learning than both experimental groups. The same trend is also shown in the case of error scores, except that the RC group is not significantly different from the RS or the SS groups. This is so because the RC group was slower than the DWS group in learning, even though the differences do not reach significance levels. There is no apparent reason why the RC group should be slower in learning than the DWS group. The only explanation that seems plausible is that the subjects in the DWS group were able to build up a 'performance set' for their learning during the pre-learning satiation treatment. The subjects in the RC group, however, lacked such a performance set, since they were presented the learning task immediately after their arrival in the laboratory. Such a performance set might by itself facilitate learning and account for the difference between the DWS and RC groups. Furthermore, the DWS group should be considered the more appropriate control for comparison with the RS and SS experimental groups since all three groups had the same opportunity to develop a performance set. Subjects of the RC group had no pre-learning treatment and can logically be excluded from such comparisons.

The results of this experiment confirm the earlier findings (Kanungo, Lambert & Mauer, 1962) that pre-learning satiation treatment of highly meaningful verbal units reduces their meaning and thereby produces negative transfer in the learning situation when the satiated units are used in the learning task. However, the reduction of meaning through satiation treatment prior to learning cannot explain why the stimulus and response satiation conditions did not result in any differential rate of paired-associate learning. If reduction of meaning is the only factor affecting subsequent acquisition, then the SS group should have learned faster than the RS group.

However, the lack of a reliable difference between the two experimental groups in their rate of learning can be explained in terms of an associationistic interpretation of semantic satiation presented by Kanungo *et al.* (1962). This interpretation assumes the simultaneous operation of two processes during continual verbal repetition of a word: the development of a habit to connect the repeated word with itself, conceptualized as a positive reaction tendency, and a decrease in meaning. These two factors, the word-word habit and a meaning decrement, may have a joint effect on learning. When the response words are satiated, it is the change in the meaning which accounts for the whole of the negative transfer observed, a conclusion in line with the importance attributed to meaning of the response members of paired-associate lists (Underwood & Schulz, 1960). When the stimulus words are satiated, part of the negative transfer observed can be accounted for in terms of a meaning decrement, and part of it can be explained in terms of the word-word habit formed during the satiation treatment. The argument for the possible negative transfer effect of a word-word habit on subsequent learning, when stimulus members are satiated, is compatible with Osgood's 'analysis of the relations among successively practiced activities'. Discussing the phenomena of transfer, Osgood (1953) argues that a response variation transfer paradigm, where responses are varied and stimuli are functionally identical, should yield negative transfer. The stimulus satiation condition in the present experiment is analogous to a response variation transfer paradigm and, therefore, part of the negative transfer observed in this condition can be attributed to the which should yield negative transfer as compared with a control condition wherein words that are given satiation treatment do not enter into the learning task.

(3) When response items of low meaningfulness are given satiation treatment, the design is analogous to a stimulus variation transfer paradigm which should yield positive transfer as compared to the control.

Subjects

Thirty-six undergraduate students served as subjects. None had previously participated in any psychological experiment.

Material and apparatus

Twenty two-syllable nouns were chosen from Noble's list (1952) on the basis of low meaningfulness, and two lists of paired-associates (list 1 and list 2) each containing five pairs, were prepared. Care was taken to equate the meaningfulness of the stimulus and the response members of each list. Each list was typed in six different random orders on a roll of paper in a manner suited to the standard anticipation procedure using a memory drum. A 3 sec. interval separated the presentation of the stimulus and the stimulus–response pair, and a similar interval elapsed before the presentation of the next stimulus. The inter-trial interval was 6 sec.

Another five two-syllable nouns were chosen as controls, again from Noble's list, on the basis of low meaningfulness. The average m value of the control words was matched with the average m value of either the five stimulus or the five response words of list 2. These words were used as controls in the sense that they were not to enter into the learning task after they had been given satiation treatment.

The semantic differential scales used in this experiment were the same three used in Expt. I. Each word of paired-associate list 2, each of the control words, and each semantic scale was printed on a separate 3 x 5 index card All cards were placed in a Kardex folder to be exposed in a predetermined random order one at a time, as was done in Expt. I.

Procedure

The design and the procedure was similar to that of Expt. I, except that the Retest Control (RC) group was eliminated. Furthermore, there was no need to counterbalance for the word position effect since the items in the stimulus and response positions of any pair in list 2 were perfectly matched for their initial meaningfulness. Each subject was tested twice. In the initial stage, the subject learned list 1 to a criterion of three consecutive successful anticipations. Three groups were formed, equated on the basis of their learning measured on the initial test. In the main stage of the experiment, two of the three groups served as experimental groups, a Stimulus Satiation (SS), and a Response Satiation (RS); the other group served as the control, the Different Word Satiation (DWS) group.

In the main stage of the experiment, which started a week after the initial testing, the normal semantic profiles were taken of each of the stimulus words or of the response words of list 2 or of the control words for subjects in the SS, RS, and DWS groups respectively. Following this, each subject received the satiation treatment for those words he had already rated on the semantic scales. Thus, each subject of the SS group was given satiation treatment on the stimulus words of list 2, while the subjects of the RS and the DWS groups were given satiation treatment on the response words of list 2 and the control words respectively. The procedure was exactly the same as that described for Expt. I.

Immediately after the satiation treatment, each subject of the SS, RS, and DWS groups was presented list 2 on the memory drum with standard instructions for the anticipation procedure. The learning criterion used was three consecutive successful anticipation trials.

Results and discussion

The data were treated in exactly the same manner as for Expt. I. A one-way analysis of variance performed on both the trial and error scores of the initial test revealed that the three groups had been equated for their initial learning measures.

The analysis of variance calculated for the semantic ratings of the three groups of subjects, both before and after the satiation treatment, is presented in Table 5. The between conditions (before and after satiation treatment) *F* ratio is not significant. Similarly, the interaction effect is also not significant. These facts support our first hypothesis that satiation treatment applied to low meaningful verbal units should not significantly change their intensity of meanings. The between-group *F* ratio, however, is significant at the 0·05 level, suggesting that groups RS, SS and DWS are different in their semantic ratings. A closer examination of the mean semantic ratings of each of the three groups, both before and after satiation treatment, revealed that the only reliable mean difference was that between the RS and DWS groups after their respective satiation treatments. The analysis of variance performed on the learning data of the main test is presented in Table 6. It reveals that there is no evidence, either in the case of trial or error measures, of any significant difference

Table 5. *Analysis of variance of semantic ratings of three groups of subjects before and after satiation treatment, Expt. II*

Source of variation	Sums of squares	D.F.	Mean square	*F*
1. Between groups	64·656	2	32·328	4·603*
Between subjects in the same group	231·737	33	7·022	—
Total between subjects	296·393	35	—	—
2. Between conditions (before and after satiation treatment)	0·16	1	0·16	0·586
1 × 2	0·447	2	0·223	0·816
Pooled subjects × 2	9·013	33	0·273	—
Total within subjects	9·62	36	—	—
Total	306·013	71	—	—

* $P < 0·05$.

Table 6. *Analyses of variance for learning data of the main test, Expt. II*

		Trial measures			Error measures		
Source of variation	D.F.	Mean square	*F*	*P*	Mean square	*F*	*P*
Between groups	2	94·36	2·75	< 0·10	155·75	1·31	N.S.
Within groups	33	34·26	—	—	203·58	—	—
Total	35						

Table 7. *Analyses of covariance of the performance of three groups of subjects, Expt. II*

Learning measures	Source of variation	Sums of squares	D.F.	Mean square	*F*	*P*
Trials	Adjusted means	188·581	2	94·290	4·221	< 0·03
	Within groups	714·788	32	22·337		
Errors	Adjusted means	441·168	2	220·584	2·29	N.S.
	Within groups	3082·221	32	96·319		

in the rate of learning as a result of the different pre-learning treatments given to different groups. The F ratio for the trial measures only approaches significance ($P < 0{\cdot}10$). To increase the precision of the test by controlling the initial differences in the learning ability, a covariance analysis was performed. As will be seen in Table 7, the analysis of covariance resulted in a significant difference between the three groups with respect to the trial measures ($P < 0{\cdot}03$), whereas no difference could be obtained for the error measures ($P < 0{\cdot}20$). Furthermore, the within-group correlation coefficient, in the case of trials, is 0·61, and in the case of errors, is 0·63, both significant at the 0·01 level. This again suggests that subjects who were fast learners in the initial test tend to be the fast learners in the main test as well. Thus, with initial trials statistically equated for the three groups, different pre-learning treatments did result in different rates of learning. Error measures, however, failed to reveal such a relationship.

Since the F value for the trial scores was significant, the adjusted criterion mean of each group in the main test was computed; these mean learning scores are as follows:

Group	Mean no. of trials	Mean no. of errors
RS	10·61	16·33
SS	15·91	24·42
DWS	11·65	19·00

To ascertain the significance of the mean differences in the trial scores between any two of the three groups, the critical difference of 3·13 was calculated, which is significant at the 0·05 level. The means quoted above reveal that the SS group is significantly lower in learning (as indicated by the trial scores) than either the RS or the DWS group. This supports the second hypothesis that the pre-learning satiation treatment given to the stimulus items should yield negative transfer as compared to the control. Such negative transfer cannot be attributed to the decrement of meaning of the stimulus items during the pre-learning satiation treatment because the satiation treatment did not produce any significant meaning decrement (see Table 5). Besides, the SS group was not significantly different in its semantic ratings from the control DWS group before or after the satiation treatment, as the mean differences never exceed the critical level of 2·21. Thus it seems that the negative transfer obtained in the case of the SS group can possibly be attributed to the transfer of the positive reaction tendency of the word-word habit developed during the satiation treatment to the learning situation. There is also no significant difference between the mean trial scores of the RS and the DWS groups. This, however, does not support the third hypothesis which predicted that the response satiation condition, being analogous to a stimulus variation paradigm, should yield positive transfer as compared with the control. Here it may be pointed out that a stimulus variation transfer paradigm results in positive transfer through stimulus generalization, and such generalization should occur when there is a similarity between the practice and the test stimuli. In other words, if one first learns A–B and immediately after learns C–B, then the amount of positive transfer from A–B learning to C–B learning will depend upon the amount of similarity between elements A and C. In the present experiment it is assumed that a word-word habit develops during the satiation treatment of the response items. For instance, one of the response items was KUPOD. During the pre-learning satiation, the KUPOD–KUPOD connexion gets strengthened. In the learning situation the subjects learned WELKIN–KUPOD. The amount of positive transfer, in this case, would depend upon the similarity between the words KUPOD and WELKIN functioning as stimulus members of the practice and the test phases respectively. Since the words WELKIN and KUPOD are not similar, little positive transfer should be expected in the present experiment. This is possibly the reason why the RS group did not differ significantly from the DWS group in the learning of list 2. The results of this experiment, therefore, demonstrate that the development of a word-word habit has a greater influence on the stimulus than on the response members of paired-associate lists. That is, satiation treatment given to the stimulus items produces negative transfer via transfer of the word-word habit generated during the treatment, but similar treatment to the response items does not seem to have any effect on learning which can be attributable to this habit factor.

The results of both experiments indicate that the two factors, the development of a word-word habit and a decrement in meaning, have differential effects on the stimulus and response positions in paired-associate learning tasks. A decrement in the meaning of the responses produces greater variance in learning performance as compared to a similar decrement in the meaning of the stimuli. However, an increase in the strength of the tendency to connect the stimulus with itself produces great change in the rate of subsequent learning as compared to the similar habit strength of connecting the response with itself.

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DEVELOPMENTAL FACTORS IN READING AND WRITING BACKWARDNESS

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Two groups of patients referred on account of backwardness in reading or writing or both were studied. These were selected on the basis of a discrepancy of more than 20 points between 'verbal' and 'performance' I.Q.'s on the W.I.S.C. or W.A.I.S., the verbal I.Q. being the lower in group 1 (6 cases) and the performance I.Q. the lower in group 2 (7 cases). Other clinical evidences of language disorder were found in group 1, but were almost completely absent in group 2. A specific difficulty with tests of finger differentiation and order, as well as impairment on constructional tasks and mechanical arithmetic, was found in group 2, but not group 1. These two groups represent syndromes of developmental cerebral deficit based on difficulties in sequential ordering and in the language sphere respectively. Each syndrome may give rise to a characteristic type of delay in learning to read and write. Children with such cerebral cortical deficits probably represent a minority within the population of retarded readers and writers.

INTRODUCTION

Some children of normal intelligence meet unexpected difficulties in learning to read and write. These difficulties have been explained in a wide variety of mutually exclusive ways.

Morgan (1896) postulated 'congenital word-blindness' as an isolated defect of cerebral cortical functioning. Subsequently many others, though repeatedly renaming the condition, and differing as to the nature of the underlying functional disorder and the extent to which it may be regarded as 'pure', or 'isolated', have agreed that 'There exists, within the community of poor readers, a specific syndrome wherein particular difficulty exists in learning the conventional meaning of symbols—[which] is of constitutional and not of environmental origin, and is often genetically determined' (Critchley, 1961).

This syndrome has been regarded as the result of a 'developmental lag with ultimate spontaneous maturation' (Olson, 1949) of the dominant cerebral hemisphere in the region of the angular gyrus in 'otherwise normal and undamaged brains' (Hinshelwood, 1917), though in the absence of anatomical verification this remains purely speculative. The resulting impairment of function has been regarded as limited to reading and writing (Hinshelwood, 1917; Orton, 1937), or as related to retarded development in the language sphere (McCready, 1910; Ewing, 1930). It has been explained as due to a more general disorder of visual perception, in terms implying acceptance of Gestalt hypotheses (Schilder, 1944; Drew, 1956; Hermann, 1959), or as due to defective visual discrimination (Bronner, 1917), auditory discrimination (Tamm, 1943), or both (Fildes, 1921).

Others, admitting a constitutional basis for selective difficulty in learning to read and write, have regarded it as representing one extreme of normal variation (Meyer, Nørgaard & Torpe, 1943; Malmquist, 1958). This attitude has been thought to 'dispose' of the concept of congenital word-blindness (Collins, 1961), a view based upon the naïve assumption that impairment due to disease states is necessarily total and irremediable (Burt, 1935; Murray & Gulliford, 1960). A contrasting interpretation in terms of emotional disorder was offered by Gates (1922), Blau (1946), and Lynn (1957), while multifactorial causation was invoked by Robinson (1947). Ilg & Ames (1950) and Collins (1961) looked beyond the individual to his social setting, and the latter pointed out that 'more slow learning is due to bad teaching than is generally admitted'.

Most workers have searched for some abnormality common to a group of children with selective retardation in reading and writing. But outside the fact of reading and writing retardation, which by definition is common to these children, and cannot in itself be regarded as evidence for any view of causation, it has not proved possible to isolate any common distinguishing factor which could not be regarded as merely secondary to the educational deficit. This suggests that the groups of backward readers (variously defined and selected by different workers) included children not of one type but of several, so that the test procedures were being applied to a heterogeneous population. Test results might then fail to reach significance on account of dilution of the relevant population by one or more contrasting types of case.

If backwardness in reading and writing in any given case may be due to one or more of several causes then the need is for test procedures by means of which the various causes can be distinguished. A suitable test procedure would be applicable to children of school age, and standardized on this age group. It would not be empirical, but based on present knowledge of psychological function. It would not tap abilities which might be secondarily affected by the fact of reading retardation, but would relate to some aspect of function uninfluenced by the level of achievement in reading. Preferably it would not appear to bear on the child's educational disability, thus minimizing the spread to the test situation of emotional resistances.

Among the syndromes of cerebral cortical disorder in adults is that manifested as difficulty in naming the fingers ('finger agnosia'), right-left discrimination, writing and calculating (Gerstmann, 1924). Analysis of 'finger agnosia' has shown it to be based on an inability to differentiate between the fingers in terms of their relative positions on the hand (Kinsbourne & Warrington, 1962). The essential ingredient of the writing difficulty relates to the order of letters within the word (Kinsbourne & Warrington, 1963*b*), and of the difficulty in calculation of the rank of the numbers as determined by their relative positions (Stengel, 1944). In each instance the difficulty relates to a task involving sequential ordering. These symptoms find their parallels in some children with reading retardation. Thus in such children 'the major interference with the process of recognizing or recalling the word is a failure to reproduce the exact order of its constituent letters' (Orton, 1937). Right-left disorientation in backward readers has been frequently described (Hermann, 1959; Gooddy & Reinhold, 1961), and 'congenital word blindness' has been regarded as 'analogous' to the Gerstmann syndrome in adults (Hermann & Norrie, 1958). If some children have trouble in learning to read and write on account of specific difficulty in sequential ordering, they might also have finger agnosia, the most characteristic element of the Gerstmann syndrome.

Tasks involving the naming of fingers (Gerstmann, 1924) are unsuitable for use with children who cannot normally be relied upon to know these names (Hermann, 1959). Tasks of 'finger localization' (Strauss & Werner, 1939; Benton, 1959) have not been shown to bear any relationship to the clinical sign of finger agnosia. The tests of finger differentiation and finger order of Kinsbourne & Warrington (1962), however, were shown by them to be specific for the diagnosis of finger agnosia in adults. They were then standardized on a group of children in a primary school. Success on these tests was achieved by more than 95 % of the children over the age of 7 years (Kinsbourne & Warrington, 1963*a*). These test procedures therefore seemed suitable for the detection of finger agnosia in children with reading and writing retardation who are over seven years of age.

Children with reading or writing retardation who fail on the test of finger differentiation and order would be predicted also to exhibit other elements of the Gerstmann syndrome. They might show a particular type of difficulty in reading or writing which would distinguish them from patients who are backward in reading and writing for other reasons. In particular, they might show selective impairment on those W.I.S.C. subtests with high 'spatial' loading (Maxwell, 1959).

Children who succeed on the tests of finger differentiation and order might have reading or writing retardation because they are emotionally disturbed, inadequately taught, or have a cerebral deficit of other than Gerstmann type. Again, correlations within patient groups might help in this differentiation. In particular, retardation in language development would be expected to delay the acquisition of reading and writing skills (Ingram, 1959). Indeed, in so far as reading and writing make as much demand on the language abilities of a young child as anything else he is likely to encounter, reading and writing retardation might prove to be a sensitive index of language impairment. Such a deficit should be detectable by clinical assessment of the patient's speech reception and expression, and might also result in selective difficulty on those W.I.S.C. subtests with high verbal loading (Maxwell, 1959).

These possibilities were tested by selecting from a series of patients retarded in reading or writing those who showed a wide verbal-performance discrepancy on the W.I.S.C. (or W.A.I.S.) and determining whether the direction of discrepancy correlates with success or failure on the finger tests, and on other tasks relevant to the syndromes under consideration.

Clinical study

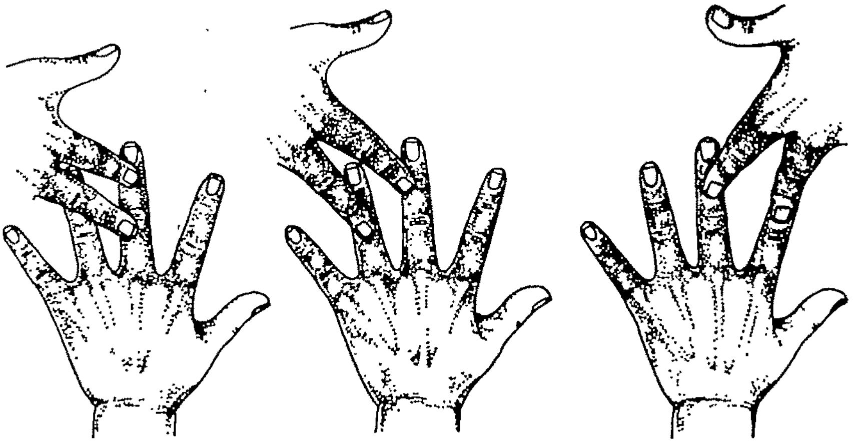
Subjects

The patients in this series fell into two groups. These were, respectively, group 1, in which the score on the non-verbal section of the W.I.S.C. (or W.A.I.S.) exceeded that of the verbal section by at least 20 points, and group 2, in which the converse was true. There were six patients in group 1 and seven patients in group 2. Their ages ranged from 8 to 14 years, with the exception of one patient in group 1 who was 31 years old.

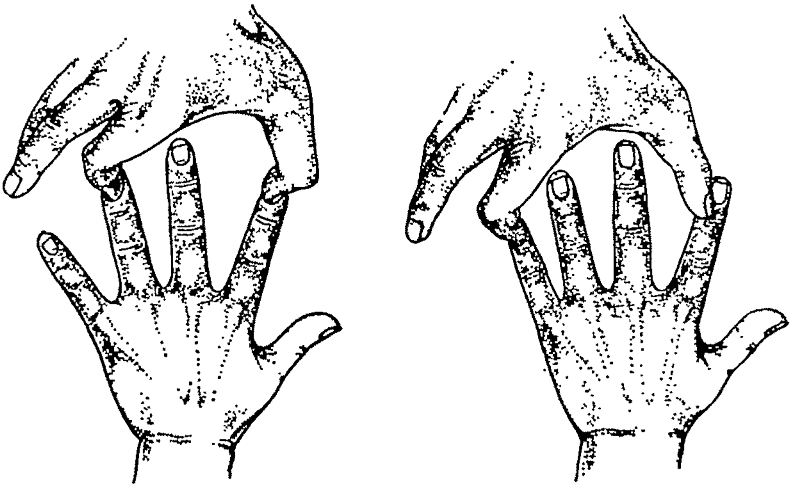
The six patients in group 1 were male; five of the seven patients in group 2 were female. Clinical history and other particulars are summarized in the Appendix.

Each patient was examined by the methods customary in neurological practice. These included routine history taking and general and neurological examination as well as such special investigations as seemed indicated. This was done under the direction of the consultant neurologist in charge of the case.

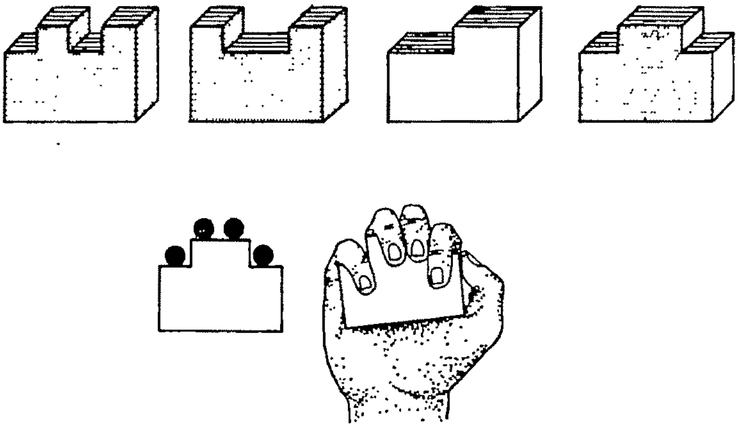
Attention was paid to the efficiency of verbal comprehension and expression (word choice, syntax and articulation). Right-left orientation was assessed by direct questioning, tasks involving personal right and left, and observation of any right-left confusion during the test interview. Constructional skill was assessed by the ability to copy drawings and matchstick patterns. The procedures were comparable with those in use for the examination of cerebral cortical function in adults (Critchley, 1953). As these tests are as yet largely unstandardized in children, the results represent no more than a clinical impression.



Test 1



Test 2



Test 3

Fig. 1

Standard procedures

(*a*) Wechsler Intelligence Scale for Children (details of tests administered are given below). The one adult in the series was tested on the Wechsler Adult Intelligence Scale.

(*b*) Tests of graded reading and spelling (Schonell & Schonell, 1956).

(*c*) Tests of finger differentiation and order (Fig. 1). These three tests are described in detail by Kinsbourne & Warrington (1963*a*).

In test 1, two points on the fingers are simultaneously touched. These may be on one, or on two adjacent fingers. The patient is required to say whether one or two fingers are being touched. Touch is maintained till a response is made.

In test 2, two fingers are simultaneously touched and the patient is required to say how many fingers there are *between* the ones being touched. Touch is maintained till a response is made.

In test 3, four wooden blocks shaped as shown in the illustration are placed in front of the patient. While his eyes are closed, the patient's fingers are moulded around one of four corresponding blocks. He is then asked to open his eyes and, without looking at the block in his hand, to pick out the corresponding one on the table.

The tests were carried out in the standard manner described by Kinsbourne & Warrington (1963*a*). Patients who failed to satisfy the criteria for 'pass' were all re-examined on another occasion before the results were accepted for purposes of the present communication.

Results

Verbal and Performance I.Q.'s, together with individual test scores, are given in Table 1. The patients in group 1 show a discrepancy between verbal and performance scores ranging from 23 to 45 I.Q. points in favour of the performance score ('negative discrepancy index'—Seashore, 1951). Individual test scores within the performance scale are on the whole consistent. Within the verbal scale the scores on Vocabulary and Digit Span, tests which make relatively high demands on language ability (Maxwell, 1959), tend to be low.

Table 1. *Intelligence test scores and individual subtest scores*

	Group I						Group II						
	1	2	3	4	5	6	7	8	9	10	11	12	13
Verbal I.Q.	72	105	75	86	101	77	89	97	81	128	84	116	86
Performance I.Q.	107	150	107	115	142	101	64	62	58	108	53	93	58
Subtest scores													
Information	6	11	4	9	11	—	4	10	6	16	5	12	7
Arithmetic	6	13	5	8	10	5	4	5	8	12	8	12	4
Similarities	8	16	10	7	13	7	13	11	7	16	10	13	10
Vocabulary	4	9	6	6	9	7	14	13	7	15	5	13	6
Digit span	6	10	5	9	9	6	—	—	5	13	10	—	7
Picture completion	9	19	12	14	14	10	10	5	5	15	10	10	9
Picture arrangement	7	15	14	14	15	9	3	6	5	14	3	8	2
Block design	10	20	9	12	17	11	4	3	6	10	5	9	5
Object assembly	16	20	9	11	18	—	2	4	0	6	0	—	0

The patients in group 2 show a discrepancy ranging from 20 to 35 I.Q. points between the verbal and the performance scores, in favour of the verbal score ('positive discrepancy index'—Seashore, 1951). Within the performance scale, the scores on Block Design and Object Assembly were the lowest in every case. These are tests with high spatial loading (Maxwell, 1959). In this group the discrepancy between the

Verbal and Performance I.Q.'s holds in spite of the disproportionate difficulty some of these patients showed on the test of arithmetic.

Table 2 presents the Reading and Spelling ages (Schonell & Schonell, 1956) and the Arithmetic ages (calculated from the score on the W.I.S.C. subtest of arithmetic). In group 1, the 'language retarded' patients, the mean reading and spelling ages are very nearly the same, 5·9 and 5·8 respectively; while in group 2, the patients with selective difficulty on performance tests, the mean spelling age is lower by one year than the mean reading age. The mean arithmetic age in group 1, 9·9 years, is very much higher than either the mean reading or spelling age. This is not the case in group 2, where the mean arithmetic age is very nearly the same as the mean reading age. It is not possible to compare the absolute values between the two groups, as the groups are not matched for severity of disability.

Table 2. *Educational attainment levels and chronological age*

	1	2	3	4	5	6	Mean	7	8	9	10	11	12	13	Mean
Chronological age	14·8	9	13·6	11	10·6	31	—	9·5	8·5	10·3	9·6	14·6	11·7	12·1	—
Reading age	6	7½	6½	6	7	5	5·9	7	6	6	12	8	8	11	8·3
Spelling age	5	7½	6	6	7	7½	5·8	8	5	6	9	7	8	9	7·4
Arithmetic age	10	10½	8½	10	10½	—	9·9	6	6	8½	10½	11	12	7¾	8·7

Table 3. *Summary of clinical psychological tests*

	1	2	3	4	5	6	7	8	9	10	11	12	13
Language functions													
Expressive	+	+	+	+	+	+	–	–	–	–	–	–	–
Receptive	+	–	–	–	+	+	–	–	–	–	–	–	–
Constructional ability	–	–	–	–	–	–	+	+	+	+	+	+	+
Writing ability (letter formation)	–	–	–	–	–	–	+	+	–	+	–	+	–
Right-left orientation	–	–	+	–	–	–	+	+	–	–	–	+	+
Addition and subtraction	–	–	+	+	–	–	+	+	+	+	+	+	+
Finger differentiation and order	–	–	–	–	–	–	+	+	+	+	+	+	+

+ Indicates the presence of abnormality.

Table 3 presents the clinical psychological findings and the result on the tests of finger differentiation and order. All patients in group 1 showed evidence of language impairment on tests of naming and verbal learning, and three of these patients showed impairment on tests of speech reception. In contrast no patient in group 2 had such difficulty. Patients in group 2 all had difficulty on constructional tasks while no patient in group 1 had this difficulty. Though all the patients in group 1 were poor at spelling, they were able to write neatly and copy written words. Four patients in group 2 had impaired script in that they were unable to form and copy letters properly. The arithmetic age quoted in Table 2 was based on the ability to solve arithmetical problems. To test mechanical arithmetic, the patients were asked to perform written additions and subtractions. Two patients of group 1, and all in group 2, found this difficult. Cases 9, 10, and all in group 2 had more difficulty with additions and subtractions than in solving arithmetical problems. One patient in group 1 was disorientated for right and left on himself, as were four patients in group 2. Systematic reversals (Benton, 1958) were not found. No patient in group 1 failed the tests of finger differentiation and order, while all the patients in group 2 failed these tests.

There was a high incidence of left-handedness and mixed laterality in both groups. However, three patients in group 1 were fully right-handed. In three patients there was a family history of reading or writing backwardness. Two of these were in group 1. In six patients there was a history suggestive of birth injury—five of these were in group 2. Motor development was normal in all but one of the six patients in group 1 but delayed in three of the seven patients in group 2. Speech development was delayed, or very delayed, in the five patients in Group 1 about whom this information was available. In contrast, speech development was delayed in only one of the patients in group 2.

Three patients were entirely normal neurologically. These were all in group 1. Three further cases were normal on examination but not electroencephalographically. Two of these patients were in group 2. There was a higher incidence of neurological abnormality in group 2 than in group 1.

DISCUSSION

A group of patients who showed selective impairment on the performance subsection of the W.I.S.C. also failed the standard tests of finger differentiation and order (group 2), and had difficulty in arithmetic, while the other group (group 1), who did less well on the verbal subsection of the W.I.S.C. (or W.A.I.S.) than on the non-verbal, passed tests of finger differentiation and order and showed no selective impairment of arithmetical abilities. Furthermore, clinical testing revealed significant impairment of language ability, outside reading and writing, in group 1.

An analogy may be drawn between these two groups and two syndromes of cerebral cortical disorder in adults, the Gerstmann syndrome and aphasia. In group 2 the finger agnosia of the Gerstmann syndrome is represented by poor performance on the tests of finger order and differentiation; significant retardation in right-left orientation and in mechanical arithmetic was also found, as well as constructional difficulty comparable to that which accompanies the Gerstmann syndrome in adults (Ajuriaguerra & Hécaen, 1960). On the other hand, the cases in group 1, in their delay in acquisition of speech and in their clinically apparent difficulties in verbal comprehension and expression, provide a picture analogous to that of aphasia in the adult.

It appears that among backward readers and writers there exist two groups with developmental defects reminiscent of acquired cerebral syndromes in adults, which will be called the 'language retardation' (group I) and the 'Gerstmann' (group II) group.

The two groups are readily distinguishable by use of the finger tests. The 'Gerstmann' group is further characterized by a positive discrepancy score (Seashore, 1951) on the W.I.S.C. greater than that occurring naturally in 95 % of the population. This cannot be regarded as a secondary consequence of failure to acquire reading skill, as this, if anything, gives rise to a discrepancy in the opposite direction, with performance scores exceeding verbal scores (Strang, 1943; Holmes, 1954; Vernon, 1957; Collins, 1961). Indeed Rabinowitch, Drew, De Jong, Ingram & Withey (1954) were so sure that the retarded readers would have a lower verbal than performance I.Q. that they excluded from their study all children who did not fulfil this criterion.

The selectively depressed verbal scores of the 'language retardation' group could more easily be regarded as non-specific and secondary to the lack of reading skill. However, the marked degree of discrepancy, in association with a history of retarded speech development, and in some cases persisting evidence of inadequate verbal expression and reception, distinguishes this group from the rest. With regard to their discrepancy index on the W.I.S.C. these patients resemble those of Rabinowitch *et al.* (1954) and clinically they conform to the description by Ingram (1959) of 'developmental dyslexia' secondary to retardation of language development. The analogy between these syndromes of acquired cerebral deficit in adults and developmental cerebral deficit in children is solely at the level of function, with due allowance for the widely different settings involved—the loss of an ability previously possessed, as opposed to the failure to acquire that ability. No anatomical correspondences may be assumed. The fact that a lesion in a certain part of the brain may cause a particular syndrome in the adult in no way implies that the child with an analogous developmental syndrome has a similarly localized cerebral lesion, or indeed any gross cerebral damage at all. Nevertheless, at the functional level the similarities are striking. Sequential ordering may be regarded as one of the basic activities of the cerebral hemispheres (Lashley, 1951), and it appears that the ability to recall and manipulate sequences (at least in relation to the fingers), is not as a rule developed much before the age of 6 years (Kinsbourne & Warrington, 1963*a*), nor even much later in some instances (the present work, group 2), and is impaired by damage to the dominant parietal lobe in some adults (Kinsbourne & Warrington, 1962). Thus the young child, the older child with the developmental Gerstmann syndrome and the adult with the acquired Gerstmann syndrome, all lack the same basic ability, each in a quite different setting and with somewhat different consequences.

A different ability, this time in the verbal sphere, was delayed in its full development in the cases in group 1. If the ability, once acquired, is then lost on account of a disease process in adult life, aphasia will result. Again, the same disorder of function will express itself differently in the different setting.

The full syndrome need not be present in every 'Gerstmann' or 'language' case. The results will depend not only on the deficit (which may be at any level of severity) but also on the compensation to it. This may be so good as to permit above average reading ability in a case with the developmental Gerstmann syndrome (case 10). Nor should the arbitrarily selected criterion of a 20-point or greater discrepancy in favour of the W.I.S.C. verbal subscale be regarded as essential for the diagnosis. Less striking cases exist. These may nevertheless also show defective performance on verbal (group 1) or finger (group 2) tests.

Conversely, the presence of such a discrepancy is not in itself sufficient reason for necessarily expecting the finger tests to be failed, and diagnosing the developmental Gerstmann syndrome, although the seven cases of this series all happened to fall into this category. There are other conceivable disorders (of perception as well as of movement) which might lead to difficulty in certain performance tests, and also embarrass reading or writing. These have still to be looked for.*

Which, if either, of the two syndromes corresponds to the condition variously named 'congenital word-blindness', 'specific dyslexia', etc.? If the condition is regarded as strictly limited to the use of visual symbols (Hinshelwood, 1917; Orton, 1937) then neither group may be thus identified. Indeed, the existence of such a pure defect would be more readily acceptable had its protagonists given details of test results outside the reading and writing spheres. But even if it is agreed that the condition is found in a setting of associated defects (Hermann, 1959; Critchley, 1961) the answer is still not obvious, as each group contains features which have at times been stated to characterise specific dyslexia. Thus, selective impairment on the 'spatial' subtests of the W.I.S.C. was stressed by Galifret-Granjon (1952) and Lachmann (1960), while selective impairment on the verbal scale was found by Rabinowitch *et al.* (1954) and Drew (1956). Right-left disorientation has been frequently observed in children with reading retardation. The association of delayed speech development with reading retardation was asserted by McCready (1910). It is therefore hardly possible to select either group as representing the syndrome of specific dyslexia or congenital word-blindness. It was the failure to distinguish between two quite different types of developmental cerebral deficit which may cause reading and writing retardation that has led to the attribution of such a large variety of associated defects to children with this educational disability, and, at the same time, has made it difficult for the results of systematic testing for these deficits to reach significance. Rather than to regard specific dyslexia as a single condition, it would seem preferable to accept the occurrence among the population of backward readers and writers of cases based upon developmental cerebral deficit. In so far as the acquisition of reading and writing skill is a complex procedure, involving a variety of cerebral functions, it is not surprising to find that retarded development of one or other of the functions subserved by the cerebral hemispheres may delay this acquisition, and do so in different ways, depending upon the exact nature of the function which is insufficiently developed. The 'Gerstmann' and the 'language retardation' type of case should be regarded not as representing a single disease condition, but rather as each corresponding to a syndrome of retarded cerebral development, however caused. Among the patients here studied six suffered perinatal trauma; the others may be regarded as delayed in development of the relevant functions on account of constitutional factors ('biological variation'—McCready, 1910). The presence of brain lesions or active cerebral disease is not a necessary condition for the genesis of either syndrome, though an active disease process affecting the brain may lead to disabilities indistinguishable from those due to simple constitutional factors. The present study includes three cases in whom reading and writing deficit was a familial feature, and in three instances it may have been the consequence of delayed cerebral maturation on a genetic basis (Hallgren, 1950). Finally, although there is no such case in the present series, the two types of defect may coincide in the same patient. In such cases the W.I.S.C. discrepancies would to some extent cancel out, and the W.I.S.C. 'profile' lack characteristic features. There might be special difficulties in distinguishing them from cases with uniform mental retardation.

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* Since submitting this manuscript we have seen a nine year old boy who was quite unable to read, and, in spite of a performance deficit of 25 points, passed the finger tests.

Syndromes of cerebral cortical deficit causing reading retardation may be associated with a variety of neurological deficits or may occur in relative isolation, but it is knowledge of the nature of the underlying disorder of function, whatever its accompaniments, which is of most immediate importance for treatment. It has been questioned whether the empirical methods at present in use in remedial reading centres offer any advance over competent teaching in the first instance (Collins, 1961), and it may well be as Ilg & Ames (1950) and Collins (1961) suggest, that the bulk of children in such classes are there on account of shortcomings in their schools rather than in themselves. Children such as those described in this communication probably represent only a minority among the population of backward readers. Nevertheless, this minority might benefit if taught along lines which take cognisance of the nature of the disorder of cerebral cortical function. The present study is an attempt at a classification of backward readers and writers into groups which could then be made the subject of controlled experiments involving a variety of teaching methods.

We wish to thank the physicians and surgeons of the National Hospital for permission to study patients under their care.

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APPENDIX. *Summary of age, sex, clinical history, and neurological findings*

Group I

Case no. ...	1	2	3	4	5	6
Sex ...	M	M	M	M	M	M
Age ...	14·8	9	13·6	11	10·6	31
Dominance: Hand	L	R	L	R	R	L
Foot	L	R	R	R	R	R
Eye	L	R	L	R	R	R
Family History of:						
Reading or writing retardation	–	Father	–	–	Father	–
Left-handedness	–	–	+	–	–	++
Birth injury	–	–	–	–	+	Not known
Retarded motor development	–	–	–	–	+	Not known
Retarded speech development	++	+	+	+	++	Not known
Neurological abnormality	Motor aphasia	–	–	–	–	–
Air encephalogram	Mild left cerebral atropy	Not done	Normal	Normal	Normal	Not done
Electro-encephalogram	Diffuse dysrhythmia	Diffuse dysrhythmia	Normal	Diffuse dysrhythmia	Normal	Normal
Audiometry	Normal	R. ear normal, L. ear deaf	Not done (clinically normal)	Normal	Normal	Not done (clinically normal)
Other symptoms	–	–	–	–	–	–

Group II

Case no. ...	7	8	9	10	11	12	13
Sex ...	F	F	M	F	F	M	F
Age	9·5	8·5	10·3	9·6	14·6	11·7	12·1
Dominance: Hand	R	L	R	R	L	L	R
Foot	R	L	L	R	L	L	R
Eye	L	R	R	L	L	L	L
Family History of:							
Reading or writing retardation	–	–	–	Father	–	–	–
Left-handedness	–	++	+	–	+	–	+
Birth injury	–	–	+	+	+	+	+
Retarded motor development	+	+	–	–	–	+	–
Retarded speech development	–	–	–	–	+	–	–
Neurological abnormality	Concomitant strabismus	–	Nystagmus, visual defect left eye	Athetosis	Right homonymous hemianopia and spastic hemiparesis	Spastic diplegia	–
Air encephalogram	Enlarged lateral ventricles	Normal	R. cerebral atrophy	Not done	L. cerebral atrophy	Not done	Normal
Electro-encephalogram	Normal	Diffuse abnormality	Diffuse dysrhythmia	Bilateral abnormality, R, L	L. temporal abnormality	Diffuse dysrhythmia	Diffuse abnormality
Audiometry	Normal	Normal	Normal	Not done (clinically normal)	Not done (clinically normal)	Not done (clinically normal)	Not done (clinically normal)
Other symptoms	Emotional disorder Clumsiness	–	–	Clumsiness	Epilepsy	–	Epilepsy

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THE MAUDSLEY PERSONALITY INVENTORY USED WITH PSYCHIATRIC INPATIENTS

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The Maudsley Personality Inventory was given to an unselected group of psychiatric inpatients. All diagnostic groups were differentiated from normals on the Neuroticism scale but neither this nor the Extraversion scale distinguished between diagnostic groups. Contrary to Eysenck's claim, hysterics and dysthymics were not distinguished on the Extraversion scale.

The negative correlation between Extraversion and Neuroticism in neurotic groups and the use of the short form of the scales are discussed. Because Eysenck's original report is not readily available, some general comments are offered about the methods used to devise and standardize the test.

The results suggest that the test is unlikely to be of value in the individual case.

I. INTRODUCTION

The original article on the Maudsley Personality Inventory (M.P.I.) appeared in 1956, written in English, in the Italian journal *Rivista di Psicologia* (Eysenck, 1956). In this article, Eysenck explained the development of his two personality scales, Neuroticism and Extraversion, which together form the Maudsley Personality Inventory.

Neuroticism and Extraversion are the two important dimensions of Eysenck's theory of personality. Eysenck had noted that the Guilford–Martin *R* and *C* scales distinguished groups of patients in the manner predicted by his theory, i.e. hysterics and psychopaths (the 'extraverted neurotics') had high *R* scores when compared with anxiety states, reactive depressives and obsessionals (the 'introverted neurotics'). Both groups of patients had high *C* scores when compared with normals and, in addition, *R* and *C* scores were negligibly correlated (Hildebrand, 1953; Slater, 1960), as required by the theory of distinct orthogonal dimensions. In addition, the finding that high *R* scorers were more difficult to condition than low *R* scorers (Franks, 1956) also fitted another part of his theory of neuroses.

In spite of their value for his theory Eysenck saw in the Guilford scales the drawbacks of their length (over 200 items), their sex differences, the poor reliability of some of the items and the intercorrelation between scales. To overcome these difficulties, the M.P.I. was devised in the following manner.

A questionnaire of 261 items was prepared from the *C* (Cycloid Emotionality) and *R* (Rhathymia) scales already mentioned, the Guilford *S* (Social Introversion), *G* (General Activity) and *A* (Ascendance) scales and the Maudsley Medical Questionnaire. These items were given to 200 male and 200 female normal subjects, mainly in the 20–35 age range and approximately half with some kind of University education. All the items were then analysed for correlation with the *C* and *R* scales in turn and those items were selected which correlated highly with one of the two scales and negligibly with the other: questions which showed sex differences were omitted or their effects neutralized. In this way 48 questions were selected for the Maudsley Personality Inventory, 24 to measure neuroticism (*N*) and 24 to measure extraversion (E). Answers were *Yes*, *No* or *?* and were scored two points for an answer in the keyed direction and one point for a question mark. All the N questions were keyed for *Yes* answers, but only 16 E questions were so keyed, the others being keyed *No*. The whole scale takes only a few minutes to complete. The maximum score on each scale is 48 and the mean scores for normals (Eysenck, 1956) are for N 18·63 (S.D. 11·19) and for E 24·89 (S.D. 9·67). The split-half reliability coefficients quoted are 0·884 for N and 0·831 for E.

A short personality test with this reliability has obviously great appeal, and since its publication the M.P.I. has been used as a personality measure in a number of studies. It has, for example, been used in investigating response to drugs, rates of conditioning, delinquency, in factorial analysis of personality and in psychiatric classification. In this last field the studies have tended to concentrate upon the examination of hypotheses arising directly from Eysenck's theory, e.g. that hysterics and dysthymics are differentiated by the E scale. The clinical use of these scales in discriminating within an unselected group of in-patients has not been tested previously.

II. PRESENT STUDY

The present study was undertaken to investigate the value of the M.P.I. as a diagnostic tool in psychiatry. The test was administered to unselected groups of in-patients in the psychiatric wards of the Southern General Hospital, Glasgow, on several different occasions over a period of 6 weeks. The group administration was supervised by the psychologists at the hospital. The only grounds for excluding a patient or his answer sheet were that he had done it at one of the previous trials or that he was incapable of completing it by reason of his lack of understanding of the questions.

To avoid contamination of the diagnoses by the results, the inventories were not scored until the patients had been discharged. The diagnosis taken for each patient was that given by the psychiatrist in charge of the case and was usually taken from the patient's discharge letter. All cases where the diagnosis was stated to be doubtful were excluded, but it is realized that some wrong diagnoses may inadvertently have been included. 151 cases were finally collected in this way.

Results

Table 1 gives the N and E scores for the whole group and for each diagnostic group. None of the differences between means or variances for the sexes was found to be statistically significant and so results for the two sexes are combined when subsequently discussing the diagnoses. The scores found by Sainsbury (1960) among out-patients at medical and surgical clinics of two general hospitals provided comparison with a non-psychiatric group of patients. In fact Sainsbury's results are not significantly different from those for Eysenck's normals. In addition, the mean N and E scores of Sainsbury's psychiatric out-patients are not significantly different from the mean N and E scores of our total group. Eysenck's and Sainsbury's results are given at the foot of Table 1.

Fig. 1 shows the frequency distribution of the N and E scores for the whole group. It will be observed that a dip occurs in both the histograms near the mean of the original standardizing groups (see Table 1). A bimodal distribution of this sort can arise because of the particular technique used in selecting these questions, namely, to test by χ^2 the ratio of *Yes* to *No* answers for each question given by the 100 subjects above and the 100 below the median for each of the *R* and *C* scales. This method, in fact, favours questions that are answered *Yes* and *No* with equal frequency since this proportion gives the highest χ^2 for a given normal distribution. Provided the reliabilities of the questions are high enough this will also lead to a bimodal distribution whenever the test is used. This artefact of the standardizing technique is likely to give the impression that any group on which the test has been used is non-homogeneous.

Table 1. *Basic information and results by diagnostic group*

(Present study, Eysenck (1956) and Sainsbury (1960).)

				Neuroticism		Extraversion		
Diagnosis	Sex	f	Mean age	Mean	S.D.	Mean	S.D.	r_{NE}
Depression	M	23	50	29·1	11·3	20·4	7·9	−0·52
	F	19	46	27·3	13·5	23·8	8·7	−0·29
	Both	42	48	28·3	12·4	21·9	8·5	−0·42
Character Neurosis	M	25	43	30·2	14·4	23·0	8·7	−0·51
	F	10	38	37·1	8·4	17·4	8·0	+0·58
	Both	35	41	32·1	13·4	21·4	8·9	−0·36
Dysthymia	M	15	38	34·7	8·1	21·7	10·4	−0·12
	F	15	39	33·7	9·6	17·7	9·0	−0·14
	Both	30	38	34·2	8·9	19·7	9·9	−0·11
Hysteria	M	1	57	30·0	—	14·0	—	—
	F	11	40	28·0	14·2	19·8	8·5	−0·33
	Both	12	41	28·2	13·6	19·3	8·4	−0·33
Organic Psychoses	M	10	39	29·1	11·9	26·0	12·1	−0·47
	F	5	43	31·4	8·0	25·6	9·1	−0·00
	Both	15	40	29·9	10·8	25·9	11·2	−0·38
Schizophrenia	M	6	35	34·5	11·1	15·0	5·9	−0·52
	F	6	35	22·8	6·8	18·3	4·9	+0·49
	Both	12	35	28·7	10·1	16·7	5·7	−0·31
Mania	M	2	47	39·0	—	20·5	—	—
	F	3	23	24·0	—	23·0	—	—
	Both	5	33	30·0	8·1	22·0	4·8	−0·10
Whole group	M	82	43	31·1	12·0	21·7	9·4	−0·44
	F	69	40	30·0	11·9	20·6	8·8	−0·18
	Both	151	42	30·6	12·0	21·2	9·1	−0·32
Eysenck's normals (1956)	M	200	—	17·8	11·3	24·6	10·0	−0·15
	F	200	—	19·4	11·0	25·2	9·3	−0·04
	Both	400	—	18·6	11·2	24·9	9·7	−0·09
Sainsbury's O.P. Controls	Both	546	—	18·4	10·8	25·7	8·5	—
Sainsbury's O.P. Neurotics	Both	116	—	32·0	9·2	21·4	10·1	—

In addition, this method of choosing items can lead to more serious drawbacks, in particular to poor discrimination at the extremes of the distribution, because all the discrimination has been focused at the median point. For clinical purposes it is essential to have adequate discrimination at the extremes of the scales. Eysenck's intention was to devise a test which primarily discriminated within a normal group. For this purpose his technique is appropriate. However, this does not necessarily lead to good discrimination between normals and neurotics.

For example, he reports (1956) that the lowest χ^2 in the Maudsley Medical Questionnaire (M.M.Q.) came from items that were not answered 50 % *Yes* in the standardization. 'It would appear, therefore, that in part, at least, the unsatisfactory validity of the M.M.Q., with normal groups may be due to the gross divergences from a 50/50 distribution of answers.' But this divergence from the 50/50 distribution probably explains why the M.M.Q. discriminated better than the Guilford *C* scale between normals and neurotics, as Eysenck stated in the same article.

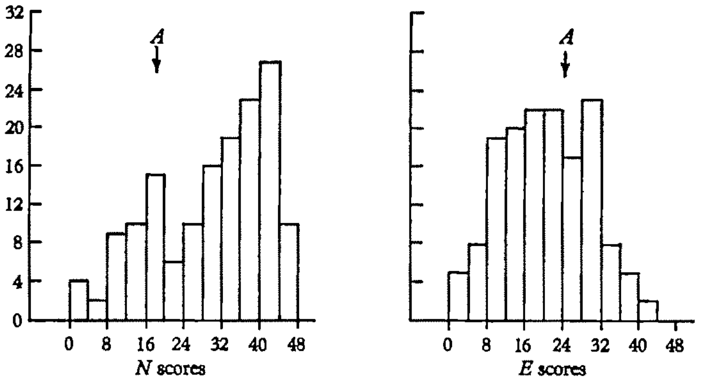

Fig. 1. Frequency distributions of *N* and *E* scores for the total group. Points *A* indicate the mean scores for the original standardizing group.

Differentiating diagnostic groups

Fig. 2 shows each diagnosis plotted on the two axes *N* and *E* according to their means so that comparison can be made with Eysenck's predictions—that there would be a division of the high *N* scorers into an introverted group, containing the dysthymics, and an extraverted group containing the hysterics and those classified as character neuroses.

On the *N* scale the biggest difference in means among our patients can be seen to be between dysthymia and hysteria. The difference of about 6 points is not statistically significant. Sigal, Star & Franks (1958) reported 35 dysthymics and 15 hysterics with mean *N* scores of 36·8 and 24·1 respectively, a difference twice as great as in the present results. There is nothing in Eysenck's theory which would suggest this result for neuroticism.

It will be observed from Fig. 2 that all the diagnostic groups have higher mean *N* scores than Sainsbury's control group and Eysenck's normals. All these differences are statistically significant. This finding suggests that the *N* scale is likely to be of value as a screening test.

On the *E* scale, the theory states that when compared with normals the dysthymics score low and the character neuroses and hysterics high. In fact, all our 3 groups were found to be below the average for normals, with hysterics lowest of all, though negligibly different from dysthymics. Other workers have found this low *E* score for hysterics (Sigal *et al.* 1958; Claridge, 1960). Eysenck (1958*a*) in reply to the former paper stated that he placed more emphasis on the relative positions of the dysthymics and hysterics, so that it did not matter if hysterics were introverts so long as they were more extraverted than dysthymics. However, the present study found no difference between these groups.

It should be noted that our hysterics have lower *E* scores than are usually reported for that diagnosis (a mean score of 19 in place of the more usual 24). This may arise from a bias in selection for admission, as this is not regarded as being the best treatment for hysterics. Thus it may be that the only hysterics admitted are those with accompanying symptoms of anxiety and depression, with consequently lower *E* scores.

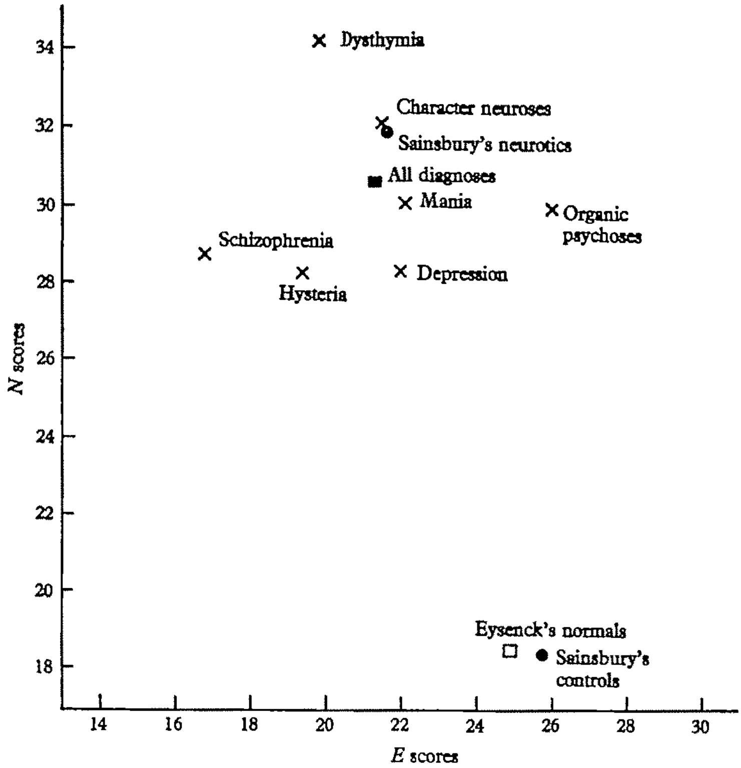

Fig. 2. Mean values of N and E scores for each diagnostic group and the whole group, as well as Eysenck's and Sainsbury's results.

Another possible explanation is that we use the classification hysteria in a more restricted way than is often done. Brengelmann (1960) admitted that more than 80 % of his 'hysterics' (mean score 24·5) were diagnosed as personality or character disorder or psychopath. All of these diagnoses we have included under character neurosis.

In the present study, of the other diagnostic groups, organic psychosis is the only group whose mean *E* score exceeded the mean for normals as predicted by Eysenck. Schizophrenia produced the most introverted group, again as predicted.

The results of the M.P.I. in this study have been found to be very disappointing. Quite apart from their failure to support Eysenckian theory, they are also seen to have, in practice, little discriminating power between diagnostic groups. Out of 21 possible comparisons between the 7 diagnostic groups for each of the scales, we found that on the *N* scale only 1 difference (hysteria *v.* character neurosis) was significant at the 5 % level and on the E scale only 2 differences (schizophrenia *v.* depression and *v.* organic psychoses) were significant at the same level. Out of a total of 42 possible comparisons for the 2 scales, a P value of less than 0·05 means by definition that differences of the observed magnitude will be found on the average in two comparisons when there is no real difference. Thus our finding that three differences reach this level of significance does not represent much improvement on this chance expectancy.

Reliability

The reliability coefficient of the test for the whole group was found to be 0·892 for N and 0·815 for E. These compare favourably with the reliability coefficients quoted in the original article (i.e. 0·884 for N and 0·831 for E). This split half coefficient was calculated using Eysenck's original splitting. However, it was observed that the two halves used for each scale are almost identical. For example, compare Q. 10 'Are you inclined to be moody?' with Q. 11 'Do you have frequent ups and downs in mood, either with or without apparent cause?' or Q. 14 'Are you inclined to keep in the background on social occasions?' with Q. 36 'Are you inclined to keep quiet when out in a social group?'. Examination of the questionnaire itself will show similar resemblances between questions 13 and 31, 15 and 33, 17 and 35, 16 and 38, 9 and 22 and others: these are the pairings used by Eysenck. This is surprising when it is remembered that one of Eysenck's reasons for abandoning the Guilford scales was their repetitiveness which added nothing to their real reliability.

Table 2. *Comparison of split-half reliabilities using Eysenck's original splitting and an alternative splitting*

	N score		E score	
	uncorr.	corr.	uncorr.	corr.
Eysenck's group (1956)	(0·793)	0·884	(0·712)	0·831
This group, Eysenck's splitting	0·805	0·892	0·688	0·815
This group, alternative splitting	0·672	0·803	0·442	0·611
Significance of diff.: $P <$	1 %	1 %	1 %	0·1 %

To determine to what extent the original reliability coefficients were spuriously high, due to this pairing, an alternative splitting was used. Since the odd-even splitting of the N scale yields the same pairing as before, this technique could not be used. Instead, the pairing was deliberately avoided by including each pair in the same half. This yielded significantly lower reliability coefficients (see Table 2). While this particular splitting represents a more stringent test than would ordinarily be required, it would have made little difference to the originally quoted coefficients if they had been measuring true reliability.

On the N scale, the greatest difference of means between any two groups is of the order of 6 points and the average standard deviation is of the order of 10 (see Table 1). For two such groups with these values, we can expect that about 25 % of the group with the lower mean will be above the mean of the higher group irrespective of numbers. Similarly, on the E scale, the maximum difference of means of about 9 and a standard deviation also of 9 indicates that there will still be about 16 % of one group above the mean of the other. It follows, therefore, that even if larger groups produced results which were significantly different in the statistical sense they would be unlikely to have any discriminating value for the individual case.

In order to investigate whether the combined N and E scores would be more discriminating, individuals of two different groups were plotted on a two-dimensional field according to their N and E scores and attempts were then made to draw a straight boundary which would separate the two groups as accurately as possible, i.e. with a minimum of misclassifications. Manipulation of various combinations of N and E by this method failed to produce any improvement in the differentiation of diagnoses.

The Interdependence of N and E

Eysenck claimed originally that the N and E scales were orthogonal (i.e. should show negligible correlation with each other) as the two dimensions they were intended to measure are claimed to be. He achieved this orthogonality by his method of selecting the question, and cross-validated the negligible correlation by testing another 200 normal males ($r = -0{\cdot}1$).

Among groups of neurotic patients, however, this negative correlation between N and E is higher, of the order of $-0{\cdot}3$ or $-0{\cdot}4$ (Sigal *et al.* 1958; Jensen, 1958; Bartholomew, 1959; Brengelmann, 1959, 1960). In our study the correlation coefficient was $-0{\cdot}32$ for the whole group ($P < 0{\cdot}001$). Further, there was a significant sex difference in this matter with for males $r = -0{\cdot}44$ ($P < 0{\cdot}001$) and for the females $r = -0{\cdot}18$ (N.S.). This was despite the fact that the sex differences for means and variances was negligible. It will be seen from Table 1 that with the exception of the dysthymic group where the correlations were negligibly different the direction of difference was the same for every diagnosis, i.e. female groups producing positive correlations in two instances and where both correlations were negative the coefficient for the males was numerically greater.

This sex difference appeared in the results of Eysenck's original standardization with $r = -0{\cdot}15$ ($P < 0{\cdot}05$) for the males and $r = 0{\cdot}04$ (N.S.) for the females. No other figures have been quoted separately for the sexes. There is no obvious reason for this sex difference but it may be related to the more general problem of negative correlation.

Of the various explanations offered for the negative correlation the first put forward by Eysenck in 1958 was that it is due to response set, because all 24 N questions are keyed *Yes* for scoring purposes and only 16 E questions are similarly keyed. He reasons that to obtain maximum scores on both scales extremely neurotic extraverts have to answer 40 out of 48 questions *Yes* and thereby develop a set to answer *Yes* so that they continue to answer in this way to even the 8 questions to which they should answer *No*. This does not happen to less neurotic extraverts or to extremely neurotic introverts and the result is a negative correlation. This would also explain why hysterics have lower E scores than Eysenck predicted.

If this explanation is true then one would expect significant differences in the contribution to total E score of the *No* keyed questions at different values of the N score. This hypothesis of response set was investigated by scoring the E scale in

2 parts, i.e. separately for *Yes* and *No* keyed questions. In this way it was possible to calculate the extent to which each individual E score was derived from *Yes* keyed answers (E_{Yes}) and from *No* keyed answers (E_{No}). On the basis of the N score the total group was divided into three approximately equal groups, and the relative contributions of E_{Yes} and E_{No} answers were obtained by finding the average for each of these components. (Columns $\bar{E}_{Yes}$ and $\bar{E}_{No}$ in Table 2.) The last column shows that there is only a very slight and not statistically significant decline in the contribution of the *No* questions to the total E score with increase of N (32, 31 and 28 %). Thus Eysenck's prediction of negative correlation due to response set is in the right direction. However, response set is by no means the whole explanation as, in addition, the mean E_{Yes} score for each N group also decreases.

Eysenck's second suggestion is that the negative correlation is due to 'positive feedback in the reaction between strong innate emotionality and conditioning processes theoretically underlying the manifestation of dysthymia' (Eysenck, 1958*a*). This is taken to mean that the more neurotic and introverted the person, the more likely he is to become phobic of social situations and therefore to withdraw from them, thereby scoring even lower on the E scale. Sigal *et al.* (1958) tested this hypothesis by matching hysterics and psychopaths on the one hand with dysthymics on the other according to their N scores, and found no difference in their mean E scores.

Table 3. *Contributions of No and Yes keyed answers to the E scores of different N groups*

N score	f	$\bar{E}$	$\bar{E}_{Yes}$	$\bar{E}_{No}$	$\bar{E}_{No}$ %
40–48	45	17·64	12·71	4·93	28·0
30–39	49	21·14	14·67	6·47	30·6
0–29	57	23·93	16·18	7·75	32·4
All	151	21·15	14·66	6·50	30·7

A third hypothesis by Jensen (1958) is that the E scale items are socially desirable traits. Highly neurotic individuals with consequently lowered self-esteem may be less willing to attribute these qualities to themselves, having less desire to make a good impression. This hypothesis is favoured as it has the added advantage of suggesting a reason for the previously mentioned sex difference in negative correlation. Being in hospital for a 'nervous' complaint represents a greater blow to self-esteem for the male than for the female. Jensen's effect is therefore more marked and the correlation becomes more negative for males.

Position of psychotics

In addition to the two dimensions of Neuroticism and Extraversion, Eysenck has postulated the dimension Psychoticism, which should be orthogonal to the first two. Psychotics should therefore be able to gain normal scores on Neuroticism. However, all four of our psychotic groups have considerably higher scores on the N scale than Sainsbury's control group and are not significantly different from the neurotic groups (Fig. 2). That psychotics score high on tests of neuroticism in general has been found in other studies (Trouton & Maxwell, 1956; Eysenck, 1955; S. B. G. Eysenck, 1956) though neurotics have normal scores on psychoticism scales. This position of psychotics is explained by Eysenck (1960) as being a neurotic reaction by non-neurotic individuals under the stress of their psychotic illness. No matter how reasonable this explanation may be, the practical result is that the M.P.I. cannot discriminate psychotics from neurotics.

Prediction from the first 12 *questions*

A short form of the test was prepared by Eysenck (1958*b*) for use where a quick assessment of personality was needed, e.g. in market research. He found that the correlations between the short scales of 6 questions with their respective full scales were almost as high as the test reliabilities.

It was decided to assess the value of this short test in psychiatric patients. The high correlations were confirmed: $r_{nN} = +0{\cdot}89$ and $r_{eE} = +0{\cdot}70$. Table 4 gives the mean and standard deviations for each of the diagnostic groups, using n and e to refer to the respective short scale scores.

It will be noted from Table 4 that the average short e scores fit the Eysenckian hypothesis better than the full E scores, as dysthymics and schizophrenics appear as low scorers while at the more extraverted end of the group are the organics, character neuroses, manics and hysterics, all as predicted. Unfortunately, the difference between hysterics and dysthymics for the short e scale still does not reach statistical significance. The first 6 E questions may be more valid measures of extraversion than later questions and are likely therefore to repay further study.

Table 4. *Scores on first* 12 *questions for diagnostic groups and for normals* (*Eysenck*, 1958)

		Neuroticism		Extraversion				
Diagnosis	f	$\bar{n}$	S.D.	$\bar{e}$	S.D.	r_{nN}	r_{eE}	r_{ne}
Depression	42	7·19	3·71	6·33	3·63	+0·91	+0·71	−0·31
Character Neurosis	35	8·06	4·13	6·71	2·55	+0·89	+0·66	−0·22
Dysthymia	30	8·90	2·99	5·97	3·05	+0·78	+0·79	+0·02
Hysteria	12	6·92	4·19	6·42	2·25	+0·83	+0·85	−0·23
Organic Psychoses	15	7·87	3·73	6·93	3·63	+0·78	+0·77	−0·16
Schizophrenia	12	7·33	3·91	5·83	2·62	+0·85	+0·68	−0·00
Mania	5	9·20	3·71	6·60	3·07	+0·67	+0·63	−0·77
Whole group	151	7·85	3·81	6·38	3·11	+0·87	+0·70	−0·20
Eysenck's normals	400	6·15	3·42	7·96	2·98	—	—	−0·05

III. CONCLUSION

This investigation of the Maudsley Personality Inventory in psychiatric patients showed that there was a considerable overlap in both Neuroticism and Extraversion scales for all diagnoses. The mean N score differentiated all our groups, including psychotics, from control patients and the whole group was not significantly different on either N or E from a previously reported group of psychiatric out-patients. The negative correlation found between N and E in other studies of patients was confirmed but Eysenck's explanation in terms of response set was rejected. The first 12 questions were found to be highly correlated with the total scores and, for the Extraversion scale, to give results more consistent with Eysenckian theory.

The M.P.I. has served a useful purpose in producing evidence to support Eysenck's theory of neuroticism, namely that it involves a continuum and not a dichotomy. However, it appears to have no value as a clinical tool. It might be better to leave the M.P.I. as it stands for use only with normal subjects in such fields as market research and personality investigations, and to devise a new, if similar, scale for clinical use by ensuring that it is standardized and validated on psychiatric patients.

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CHARACTERISTICS ASSOCIATED WITH CHRISTIAN NAMES

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An experiment is described in which subjects were asked to associate personal characteristics with christian names. For several of the names used, there was marked agreement that particular characteristics were apposite. Possible explanations for this are discussed.

Recent personal experience in having to select names demonstrated that some people feel strongly that particular names denote a particular sort of person. From an academic point of view, this may be an interesting instance of thinking in terms of stereotypes: from a more practical point of view, one wonders just how much of a part is played by associations of this kind in the complex business of choosing names for children. These seemed to be sufficient reasons to justify a psychologist inquiring farther into the situation.

An inquiry by Mass Observation (1946) in this country had shown that those who dislike their own name quite commonly say that this is because the name has unattractive associations for them, while Eagleson (1946) in the same year reported a similar finding in the U.S.A. The most systematic study on this topic was by Schoenfeld (1942), whose only interest in such associations was as a study of stereotypes. Using students in a college in the U.S.A., he set out to collect data to disprove the idea that stereotypes invariably have a 'kernel of truth'; arguing that it was impossible that names could really be associated with personal characteristics, he claimed that the kernel of truth hypothesis would be disproved if it were found that there was agreement between people when asked to make associations of this kind.

Though psychologists are often criticized for experimenting with student populations this aspect is perhaps not of great importance here, since Schoenfeld was merely seeking to prove that some people do agree when asked to associate names with personal characteristics. Where he does seem to have gone wrong was in using students who all belonged to the same college, for this could mean that he found common associations merely because most of the students concerned had the same person in mind when giving their associations. It follows that Schoenfeld neither disproved the kernel of truth hypothesis nor succeeded in proving that there is any broad and general agreement that names are associated with personal characteristics.

Regrettably, the writer was not in a position to be able to organize a national sample survey on this subject, but the methodological deficiencies in Schoenfeld's study could be avoided by asking people from a number of different communities to give associations of this kind. If results so obtained are summed together and agreement is then observed, it is fair to say that there is broad and general agreement on this topic, at least within the limitations of the names chosen and the communities selected for study. Whether or not it is reasonable to go on from this to say that the kernel of truth hypothesis is thereby disproved will be discussed later in this paper.

METHOD

Schoenfeld used an ingenious method for selecting the names to be used and the characteristics which subjects were asked to relate to the names. The present writer merely chose some names which happened to interest him, and a few characteristics which seemed likely to serve his purpose. In fact, the writer did not know of the existence of Schoenfeld's paper when he began his own experiments, but in practice Schoenfeld's more sophisticated experimental method does not seem to have been any better for the purpose and the present writer's method was certainly a lot simpler.

Subjects were each given a sheet with seventeen names listed on it, and asked to say whether they felt that each of six characteristics applied, did not apply, or if they did not know. The written instructions for this were worded as follows:

> The purpose of this experiment is to find out if each of certain names tends to be associated with a particular sort of person.
>
> For each name in the list, put a tick for each characteristic to show what sort of person you associate with this name. Tick DK (don't know) when you cannot make any association.
>
> Put a tick, for each name, in one or other of the last columns, to show whether or not you had a particular person in mind.
>
> Omit any name which is the same as your own. Enter in your *full* name, your age, and the date on which you completed the form.

The names and characteristics in question are shown in Table 1. The main criteria for selection of the names were that these should be either very common or rather unusual, and that both sexes should be represented. It was hoped that using common, or unusual, names would reduce the risk that subjects would relate their associations to particular people. The characteristics selected were intended to be subjective rather than objective, though this applies less to 'young' and 'not young' than to the others. A preliminary experiment had suggested that the replies were influenced by the order in which the characteristics selected were arranged (across the page), but not by the order in which the names were presented (down the page) and four different versions of the questionnaire were therefore prepared and distributed randomly among the members of each group of subjects, so as to cancel out any effects due to order. For all four versions, the names were listed in the same order as shown in Table 1; with versions *A* and *B*, the characteristics were listed in the same order as that shown in Table 1; in versions *C* and *D*, the order of the characteristics was reversed. In versions *A* and *C*, the replies were presented in the order 'yes, no, DK', but in versions *B* and *D* the order was 'no, yes, DK'.

The fact that subjects were given the opportunity to say 'don't know' avoided a common criticism of this type of approach—that subjects are forced to choose and that any agreement between subjects need not indicate what people 'really think'. The use of different communities together with the fact that subjects were asked to say when they had a particular person in mind would, it was hoped, allow for biases such as that in Schoenfeld's experiment.

The ten communities consisted of universities, research stations, and offices, widely spread geographically, and chosen with a view to there being almost no contact between the personnel at one and the personnel at another. Those who took part are in no sense a typical cross-section of the population.

RESULTS

Nine subjects felt unable to do the task, leaving 146 who completed forms. Table 1 shows the results and it can be seen that in many cases certain characteristics are strongly associated with particular names. Thus John is nearly always thought to be trustworthy, Robin to be young and Tony to be sociable; Agnes and Matilda are thought to be not good-looking and Agnes to be not young. Thus the first query ('do subjects have associations with Christian names and, if so, do they have similar associations?') has been answered positively.

Before going on to try to account for the similarities that occur, it is worth calling attention to some points of more general interest in this table. Thus, Ann and Anne are similar names and tend to receive the same associations: the main difference is that more rate Ann as young, but this is not a statistically significant difference. The general trend is for the socially desirable characteristics to be associated with names more often than the socially undesirable characteristics, which may reflect the kind-heartedness of the subjects or may be an accident due to the choice of names. This leads to some difficulties in interpretation: thus the fact that Grizelda received a very similar number of favourable and unfavourable associations for most characteristics may indicate that people generally had no views either way and wrote in 'yes' or 'no' more or less at random; or, because the associations differ from the usual trend, it may indicate that this name has rather unfavourable associations. With this particular name, the very high number of 'don't knows' tends to support the former theory. The trend for most names to have favourable associations can be seen more readily from Table 2, in which is shown the percentage of socially desirable characteristics out of the total of socially desirable and undesirable characteristics awarded.

For each name, the replies given by all those subjects who had a particular person in mind were compared with the replies given by those who did not. The general trend was for those who had a particular person in mind more commonly to give socially acceptable characteristics to a name, by comparison with those who had no particular person in mind. The difference was more pronounced where the name concerned tended to be rated at the socially unacceptable end of the scale for the characteristic concerned. However, if the ratings for each name on every characteristic are examined, this trend does not occur in a sufficiently high proportion of cases for it to be statistically significant ($P > 0{\cdot}10$).

It is of interest that when averaged ratings for the male and female names are compared, they appear as about equally socially desirable. Of course, this too could be an incidental consequence of the particular names selected. A comparison was made between the characteristics attributed to female names by men, and those attributed to female names by women, and a similar comparison was made for the male names. There were no important differences. The age of the subject does appear to affect responses to the 'young'–'not young' associations. Younger subjects are somewhat more likely to say that the people concerned are young.

An interesting point is that a name which is rated as being socially desirable by many subjects on one characteristic tends to be thought desirable on the other characteristics also. This occurs even when such correlations would be unlikely in real life, and is presumably one more instance of the well-known 'halo-effect'.

Table 1. *Associations between personality characteristics and christian names*

	Rachel	John	Miriam	Agnes	Grizelda	Jane	William	Margaret	Ian	Frederic	Ann	Anne	Matilda	Eleanor	Tony	Robin	Cyril	Totals
Trustworthy																		
Yes	86	110	49	86	42	92	92	94	90	60	83	78	66	69	67	79	54	1297
No	24	8	34	22	44	21	24	24	20	41	19	26	35	32	51	28	53	506
DK	35	14	61	36	57	31	24	25	31	41	38	35	44	41	25	36	38	612
DNA	—	14	—	1	—	1	6	3	2	3	6	4	—	—	1	—	—	41
NA	1	—	2	1	3	1	—	—	3	1	—	3	1	4	2	3	1	26
Kind																		
Yes	98	98	70	68	50	97	81	99	80	58	93	94	63	75	76	82	54	1336
No	27	14	31	40	46	22	29	21	28	44	17	21	44	34	38	20	53	529
DK	20	20	43	36	47	24	30	23	34	40	30	24	37	34	31	42	38	553
DNA	—	14	—	1	—	1	6	3	2	3	6	4	—	—	1	—	—	41
NA	1	—	2	1	3	2	—	—	2	1	—	3	2	3	—	2	1	23
Young																		
Yes	65	94	68	31	62	101	50	94	101	30	106	94	20	61	115	119	46	1257
No	47	15	34	92	53	21	61	22	18	83	12	27	98	55	17	12	58	725
DK	32	23	42	21	28	22	28	27	22	29	22	19	25	26	13	13	40	432
DNA	—	14	—	1	—	1	6	3	2	3	6	4	—	—	1	—	—	41
NA	2	—	2	1	3	1	1	—	3	1	—	2	3	4	—	2	2	27
Good-looking																		
Yes	79	75	62	16	60	72	41	70	86	45	85	86	12	66	90	78	25	1048
No	45	26	45	113	53	53	74	48	28	69	28	27	107	54	33	36	94	933
DK	20	31	37	16	30	19	24	25	28	27	27	27	26	23	21	30	26	437
DNA	—	14	—	1	—	1	6	3	2	3	6	4	—	—	1	—	—	41
NA	2	—	2	—	3	1	1	—	2	2	—	2	1	3	1	2	1	23

Table 1 (*cont.*)

	Rachel	John	Miriam	Agnes	Grizelda	Jane	William	Margaret	Ian	Frederic	Ann	Anne	Matilda	Eleanor	Tony	Robin	Cyril	Totals
Sociable																		
Yes	67	106	67	37	51	98	83	94	99	56	88	97	36	79	116	89	50	1314
No	41	14	42	83	54	27	34	30	26	52	32	25	72	40	22	33	68	695
DK	36	12	35	25	38	19	21	19	17	33	20	18	37	24	7	21	27	409
DNA	—	14	—	1	—	1	6	3	2	3	6	4	—	—	1	—	—	41
NA	2	—	2	—	3	1	1	—	2	2	—	2	1	3	—	3	1	23
Aggressive																		
Yes	60	33	36	62	51	38	54	36	46	60	27	32	54	43	42	32	49	755
No	60	85	73	63	56	85	69	85	72	49	91	92	61	78	80	83	71	1253
DK	24	14	34	19	36	20	16	21	24	32	21	16	31	23	22	29	25	407
DNA	—	14	—	1	—	1	6	3	2	3	6	4	—	—	1	—	—	41
NA	2	—	3	1	3	2	1	1	2	2	1	2	—	2	1	2	1	26
Particular person																		
Yes	36	69	27	50	21	54	62	73	52	42	67	53	17	45	76	56	56	856
No	97	49	105	87	106	83	70	64	76	86	57	73	113	87	57	75	74	1359
DK	9	13	10	7	14	6	7	4	12	11	14	11	13	10	9	11	14	175
DNA	—	14	—	1	—	1	6	3	2	3	6	4	—	—	1	—	—	41
NA	4	1	4	1	5	2	1	2	4	4	2	5	3	4	3	4	2	51
Totals																		
Yes	491	585	379	350	337	552	464	560	554	351	549	534	268	438	582	535	334	
No	341	211	364	500	412	312	361	294	268	424	256	291	530	380	298	287	471	
DK	176	127	262	160	250	141	150	144	168	213	172	150	213	181	128	182	208	
DNA	—	98	—	7	—	7	42	21	14	21	42	28	—	—	7	—	—	
NA	14	1	17	5	23	10	5	3	18	13	3	19	11	23	7	18	9	

Notes: DK is 'don't know', NA is 'no answer' and DNA is 'does not apply'. 'Does not apply' is the code used where the subject completing the form has this christian name.

To revert to Schoenfeld's interest in stereotypes, consistency has been found and there could be three possible explanations for it. There is the chance that the subjects knew the same person; there is the possibility that there is a kernel of truth (e.g. Johns are generally trustworthy); or there is the stereotype explanation, which supposes that people agree, but quite wrongly, on the idea that, say, Johns are trustworthy.

Where the results for all ten communities are summed together, as in Tables 1 and 2, and there is very marked agreement between subjects about certain names, the explanation that the subjects knew the same person can be eliminated, unless one supposes that some famous person, fictional or otherwise, can account for the consistency. The kernel of truth hypothesis, which Schoenfeld so blithely dismissed, could be relevant if, say, names are associated with particular age-groups, or particular social classes, and there is evidence that this happens. An examination of names published in births and deaths columns shows that some names are associated with older people and, probably, with social class. However, this could not be a universal explanation, for Robins cannot stay young forever and the name is not of recent origin, while John is a very common name among all classes and age groups and it is therefore hard to believe that it is likely to be always associated with being trustworthy—unless having been given such a name tends to make a person trustworthy.

Table 2. *The proportion of subjects attributing socially desirable characteristics, out of all those who do attribute characteristics, for each christian name*

	Percentage saying that this person is:						Average of the percentages for all characteristics
	Trust-worthy	Kind	Young	Good-looking	Sociable	Not aggressive	
Rachel	78	78	58	64	62	50	65
John	93	88	86	74	87	72	83
Miriam	59	69	67	58	62	67	62
Agnes	80	63	25	12	34	50	44
Grizelda	49	52	54	53	49	52	52
Jane	81	82	83	58	78	69	75
William	79	74	45	35	71	56	60
Margaret	80	83	81	60	76	70	75
Ian	82	74	85	74	79	60	76
Frederic	59	57	27	40	52	45	47
Ann	81	85	90	75	73	77	80
Anne	75	82	78	76	80	74	78
Matilda	65	59	17	10	33	53	40
Eleanor	68	69	53	55	66	64	63
Tony	57	67	87	73	84	65	72
Robin	74	80	91	68	73	72	76
Cyril	51	50	44	21	42	59	45
Average	72	72	63	53	66	59	

Where there is less marked agreement between subjects when all results are grouped together, the same three explanations may of course apply, but with the difference that such agreement as there is could have occurred as a result of marked consistency in one or two groups, perhaps as a consequence of particular persons known to the members of these groups having similar characteristics. To check on this, the results given by those six groups which were large enough to have such an influence were examined to see if there were significant differences between the groups, in the responses they gave, for all 43 cases in which the agreement on a name fell between 65 and 80% or between 20 and 35%. Marked agreement between members of one, or perhaps two, of these six groups, would mean that the replies given would differ quite widely between the six groups taken as a whole. In the event, in only two cases were differences between groups found to be significant at the $P < 0{\cdot}05$ level and in one case the difference was significant at the $P < 0{\cdot}01$ level; with 43 cases, this is little more than would be expected by chance. It appears, therefore, that these groups were reacting in much the same way when making these associations. Thus it seems that even where there is less agreement between subjects, such agreement as there is cannot be explained by the idea that particular groups are referring to one particular person.

More general checks on this hypothesis were possible. Nineteen subjects, representing several of these communities, all knew a particular Rachel and, since this is an unusual name (given to 9 children a year by *Times* readers, averaged over 12 years), it seemed that these subjects might agree closely about the characteristics for this name. In fact, they only agree together markedly better than other subjects when rating for one characteristic (aggressiveness), the difference in the way they distributed their ratings for this quality being significantly different (at the $P < 0{\cdot}001$ level) from the way in which other subjects distributed their ratings. In any case, 13 out of these 19 subjects claimed that they were not thinking of a particular person when giving their associations.

To sum up on this question of stereotypes, it seems as if these data, at least, cannot be explained away by saying that consistency was merely due to the subjects' all knowing the same people. This leaves two possible explanations, the kernel of truth hypothesis, which would be appropriate for some associations with age, and the idea that stereotypes, quite unfounded in fact, happen to exist. Of these two explanations, it is not possible to decide which is the more correct; one may be the appropriate explanation in some circumstances and the other in other circumstances, but this inquiry provided very little direct evidence on this.

So much for Schoenfeld's sophisticated psychological explanations. On a more naïve level, one wonders whether these people were influenced by some allegedly authoritative information which made them subscribe to these particular associations. Two possible sources would be well-known phrases, such as 'plain Jane' and 'honest John', and books which list names and their 'meanings'. Tables 1 and 2 show that Jane is rarely said to be 'not good-looking', though it is true that John is usually rated as 'trustworthy'. Assuming one can discount 'Queen Anne is dead', the writer knows of no other such sayings related to the names chosen here, so this explanation, adequate or not, has only a limited application. It is relevant too, that although the existence of these phrases is some indication that this type of thinking is not confined to psychological experiments, there is the further problem of how such phrases themselves originated.

Books which list names and their 'meanings' usually base the latter on biblical origins of the names, or on actual or conjectural etymology. An examination of some of these 'meanings' did not suggest that the responses of these subjects were influenced by them to any extent. It is true that Matilda ('mighty battle-maid') is rated as more aggressive than the average, but the three names whose 'meanings' imply gentleness (Agnes, Rachel and Frederic) are all rated as more aggressive still. Aggressiveness is the only relevant characteristic which is mentioned for these names.

CONCLUSIONS

This study has shown that people are prepared to associate personal characteristics with christian names and that, when they do so, they agree in many cases on the most appropriate characteristic for a given name. The trend appears to be a real one and not just an artefact due to an inappropriate experimental design, as had seemed possible with Schoenfeld's findings. No satisfactory decision seems possible as to whether this is mainly due to a kernel of truth or to stereotypes with almost no foundation in fact.

The other issue which provided a stimulus for making this inquiry was the possibility that such associations influence the choice of names. It is found that William comes fairly low when scaled according to the number of socially acceptable characteristics given, yet occurs in some of the '*Times*' top ten lists of names during 1948–61. John, Ian and Robin, rated as more socially acceptable, should therefore also occur in the top ten if associations have a large influence on the choice of names, but only John does so. With girls, the names at the top of the socially acceptable scale list tend also to come in the top ten lists of names. Such evidence assumes that the subjects would make choices similar to those made by *Times* readers and this may not be so, for an examination of lists of this kind suggests that the choice of names varies with region and with social class. Clearly, this inquiry cannot offer any useful evidence on this particular problem, which could be more appropriately examined by some kind of sample survey. In fact, the only conclusion of any practical value appears to be for those who are lucky enough to have been given a name such as John, or Ann, for they may be able to take advantage of the fact that if people know nothing about them other than their name, they will expect them to be nice persons.

Information on the relative popularity of names has been obtained from published lists of births and deaths announcements. In addition, certain details about rarer names have been given to the writer by Mr J. W. Leaver, who prepares such lists for *The Times* and by Mrs E. M. Wigley, who prepares such lists for *The Guardian*. The writer wishes to acknowledge their help. The writer also wishes to thank the subjects who took part in his experiment on name associations and those who helped in the analysis of these data.

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PUBLICATIONS RECENTLY RECEIVED

The Behavioral Basis of Perception. By J. G. Taylor. New Haven and London: Yale University Press. 1962. Pp. xvi + 379. 68*s.*

This work is dedicated to four persons whose influence is apparent in its design and development. They are: the late Prof. Clark L. Hull, the late Prof. Theodor Erismann, Prof. Ivo Kohler and Dr W. Ross Ashby. Hull, who died in 1952, is remembered for his dedicated and courageous efforts to provide within the behaviouristic programme, a deductive system based upon 'a relatively small number of primary principles or postulates, much as the secondary principles or theorems of geometry are all ultimately derived as a logical hierarchy from a few original definitions and primary principles called axioms'. An enormous amount of empirical data, chiefly from experiments in learning was marshalled in the three works, *The Principles of Behaviour* (1943), *Essentials of Behaviour* (1951) and *A Behaviour System* (1952); but as the reviewer has argued elsewhere, Hull, despite the considerable service to Psychology in collating so much data, did not achieve his major aim of constructing a deductive system of sufficient range to embrace human behaviour.

Yet experiments in learning are relatively easy to treat behaviouristically. The events are 'out there' and the results are readily quantifiable. Within a behaviourist programme, Taylor has attempted the more difficult task of constructing from the simplest of starting-points, and with a marked independence of nativist theories, a deductive system applied to visual perception. The underlying model for the system derives from W. Ross Ashby's concept of a multistable system (*Design for a Brain*, 1952). Much of the empirical content is associated with the interesting and important experiments carried out in Innsbruck by Erismann and Kohler with inverting lenses and distorting prisms, the experiments of Kohler and E. H. Land with colours and a number of experiments and observations by the author. The notation of set theory is used to present successive phases of the development and is linked with the mathematical derivation of some of the results in an appendix by Seymour Papert. The result is an outline of a system on empiricist lines which embraces several fundamental aspects of visual perception, including orientation within the environment, spatial perception, parallax, the perception of colour, and relationships with other modalities. Differences in results and interpretation generated by this system from the viewpoints of Gibson, Hayek, the Gestalt school, Werner and Wapner, Hebb and Piaget are discussed and are of an order which the several authors would find difficult to ignore.

Perception is a field of Psychology where many other and diverse fields of the subject meet. Sensory physiology, kinesiology, learning theory, factors deriving from the culture pattern and from the adjustment of the personality are all relevant. The inevitable result has been a vast literature, somewhat bedevilled by the qualitative diversity of the empirical data, the varieties of direct or implicit dualism involved and the consequent linguistic difficulties. To provide the simplification associated with a behaviouristic and deductive system is a major task. The reader is sometimes at a disadvantage because the matching of the results generated by the system involves either results which he has tended to overlook, or results which must be re-interpreted, as the author, during the development, was on occasion obliged to do. Inevitably too, with a new approach, there are many points where the reader has further questions. The net result is, however, a significant work, the development of an approach which is both penetrating and conducive to a re-appraisal of many fundamental issues and methods in this field.

F. V. SMITH

Behavior and Awareness. Edited by C. W. Eriksen. Durham, N.C.: Duke University Press; London: Cambridge University Press. 1962. Pp. viii + 158. 28*s.*

As symposia go, this one is more readable and instructive than most. It is a collection of six shortish papers gathered under a banner with a strange device. For some while now there have been subtle indications that the 'mind' is creeping back into psychology—operationally defined, of course. The main burden of this book seems to be that, although learning without awareness may occur in principle, we have as yet no evidence to compel assent to such an assertion. The wheel has turned full circle when the null hypothesis that the behaviourist is required to dislodge statistically is that 'awareness' is deemed present unless its absence can be proved. Poor old Watson must be making fractional antedating goal reactions in his grave, trying to clout Hull with a pure stimulus act.

The value of the contributions lies in the diligence with which this theme is pursued. We are reminded of the useful distinction to be made between learning and performance, and there are some valuable references, particularly to various review articles. Shortcomings of earlier work on verbal conditioning in the Greenspoon tradition are clearly exposed. The kind of answers you get depends on the questions you ask, and if you ask your subjects enough questions with sufficient care 'awareness' almost invariably emerges as the handmaiden of the learning of verbal contingencies.

There remains, however, an uneasy feeling that, although such methodological sophistication represents a welcome step towards a science of human as against rat behaviour, some important issues have been overlooked, side-stepped, or glossed over. Since the criterion of awareness is the ability to verbalize the correct hypothesis regarding the contingency being reinforced, we must conclude that learning without awareness does occur with animals and pre-verbal infants—not to mention spinal and decorticate preparations and victims of aphasia. Nor is even passing reference made to the physiology of these processes. The technique of continuously monitoring the threshold would, in itself, seem to be worthy of some consideration, while the reticular formation is surely deserving of a nod of acknowledgement, even if we do not wish to commit ourselves to the extent of a tug at the forelock.

That arguing from verbal conditioning to the learning of perceptual motor skills may involve a degree of risk is tacitly acknowledged, but quietly ignored. Such chestnuts as the one about learning to ride a bicycle would seem to make the soundness of that particular step more than suspect. In order to turn left the cyclist first has to turn to the right to throw himself off balance, but no one seems to be aware of this, even when acquiring the skill. In case it is argued that we are simply drawing on our own distant experience, anyone who has already learned to ride a bicycle can address himself to an even more complex skill, such as learning to execute a christiana on skis. Be introspective as you like—the essence of what is learned defies verbalization. We know what it feels like once we have learned the manoeuvre, but to pass this knowledge on to another, except by inviting him to go on trying various gross movements until it comes out right, is quite impossible. The language involved is essentially kinaesthetic, not verbal.

Many of the arguments put forward smack of the continuity–discontinuity controversy of bygone days. For 'awareness' read 'insight' and you can enjoy a draught of the old wine from a new bottle. A reconcilation between the opposing schools is effected by the discovery that hypotheses can be conditioned like any other kind of response, while the two kinds of learning, hypothesis and response, can go on side by side. Vaguely, one feels that this reconciliation took place some time ago, and what we are witnessing is the formal marriage of ideas that have been living in sin, without shocking the neighbours, for long enough to be recognized as a family unit by the Inland Revenue.

Of all the symposiasts, Verplanck appears to be most aware of where this primrose path is leading. His concept formation experiments show that even the limited assertion that at least *verbal* conditioning demands awareness is on shaky ground, no matter how thoroughly the subjects are brainwashed afterwards. There is also a hint that although 'insight' is a dirty word guaranteed to raise a symposiast's threshold, as 'hind-sight' it becomes respectable. That is, the subject may look back over a number of successful trials and then see what it was that led to success, thereafter applying the rule with 'awareness'. In these circumstances it is difficult to see why, since 'awareness' is permitted, either because it is hallowed by an operational definition or deduced by analogy from introspection, the same civility cannot be accorded to 'insight'.

Reading between the lines one begins to suspect that the whole pantomime is a kind of rear-guard action being fought, not so much against the Freudians, whose passions have long since been spent, but against a rising tide of the ideas that the Freudians misconstrued. Smoked out by common sense and logic from a rigid Watsonian line, the neo-behaviourists have retreated, or advanced, to a more flexible position. Learning with awareness is preferred to learning without, as it seems less dangerous to have truck with the conscious than dabble with the unconscious. Better the devils you know.

Possibly there is room for a Hegelian higher synthesis here. By walking backwards to Christmas we may yet discover that machines and animals have an unconscious, though not a conscious—until they learn to speak.

R. T. GREEN

Pattern and Growth in Personality. By GORDON W. ALLPORT. London: Holt, Rinehart and Winston. 1961. Pp. xii+593. 60*s*.

This book is a restatement, revised and reconsidered in the light of recent research, of Allport's approach to the problem of human individuality, originally formulated in his 1937 classic, *Personality: A Psychological Interpretation*, and subsequently developed in his numerous publications. The reviewer may be mistaken in his impression that the book has not received the publicity it deserves considering its importance, especially to the general reader. Allport, as ever, is erudite and stimulating; his grasp of historical trends and connexions is considerable. He is familar with European psychology, and one is grateful for his good English and short chapters. The book is well worth reading. Even so, the book should not escape critical examination, since its usefulness as a scientific document may lie in the critical reactions that it provokes. Some psychologists will feel uncomfortable with this mixed diet of commonsense, pseudoscience, popular clichés, and scientific data. Feelings of *déjà vu* occur less frequently than one might expect considering Allport's single-mindedness, but then only a few sentences are reprinted verbatim from his earlier work. Although the ideas in his other major publications are reviewed, the publications themselves are by no means rendered obsolete.

The problems, definitions and concepts of *Pattern and Growth in Personality* are similar to those of its predecessor; some will say, 'Too similar', and argue that Allport has merely dressed his conceptual system in fashionable facts after shopping around carefully for evidence. However, it must be understood that Allport has an 'approach to' not a 'theory of' personality. His broad, flexible, conceptual system is metaphysical, not in the derogatory sense but in the sense of defining problems and principles, and offering slogans and presuppositions which stimulate and guide research without themselves being susceptible to empirical examination. Allport, however, does not distinguish between such metaphysical assertions and those other assertions which can be verified or refuted. Particularly in the final chapter, one fears that Allport's approach is a slippery metaphysical slope from which one can either struggle towards the firm ground of scientific understanding or slide unwittingly into an abyss of pious hopes and platitudes. The final chapter is, in its way, as abstract as and as remote from the study of real persons as any factor analytic or actuarial account of individual behaviour. Only rarely does Allport name a person, and not once does he present a detailed case-study based on his approach.

If single-mindedness is Allport's most salient 'personal disposition', then perhaps his eclecticism comes a close second. It is obvious that he recognizes the importance of and is sympathetic with almost every approach to personality study. Occasionally, however, he is hostile to alternative methods, factor analysis and actuarial prediction for example, which he dismisses too easily while offering little in return; and he is too uncritical of notions which are humanistically acceptable but scientifically sterile. In his eagerness to be fair to everyone (or nearly everyone), Allport seems to have lost sight of the fact that the various approaches to the study of personality differ in merit and should be treated accordingly. He seems to adore the mystery of personality—a mystery which he has helped to create and perpetuate—and nowhere in his book is there the slightest indication of doubt that the problem is genuine; but there is a serious possibility that the problem has been misconceived and over-elaborated by philosophically-minded psychologists.

It appears that Allport tends to make out a good case for each particular issue by selecting evidence and neglecting contradictions: (*a*) On p. 34, he accepts the importance of heredity in relation to temperament but continues to assert its susceptibility to change; (*b*) on p. 81, he accepts the fact that drastic alterations in personality occur throughout life, but he is reluctant to relinquish the idea that the formative years of childhood set the pattern for adult life; (*c*) on pp. 95–6, he gives an oversimplified account of human learning and then condemns it with faint praise as having, '...a true but limited utility' because it takes no account of intention, set, and other motivational and cognitive dispositions, while on pp. 101–4 he identifies 'learning sets' with 'attitudinal sets'.

By overstressing the concept of 'personality', Allport seems to miss the real importance of the 'situation' as a determinant of conduct. The fact is that some environmental constraints elicit virtually the same behaviour from different people, so that no one behaves in a 'characteristic' way; at other times, a particular pattern of circumstances or some small detail may be critical in eliciting from one particular person a characteristic reaction. It is not surprising that Allport's skinnerian colleagues mutter about the need to know more about the situation. On p. 378, he appears to exaggerate the extent to which the average person is self-organized and unified, and to underestimate the instigating, directing, controlling, and stabilizing effects of a routine social and physical environment. On pp. 362–5, he recognizes as an apparently separate problem the need to study 'persons in situations' (which the reviewer believes to be the real problem); but he does not realize that the appropriate methods are jurisprudential and experimental—the very two fundamental methods of investigation that he chooses to ignore. It is amusing to observe that when he is forced to look for appropriate methods for studying persons as individuals, e.g. the problem of personal dispositions (p. 368) he heartily recommends what is, in effect, a combination of the jurisprudential and experimental methods.

Although he devotes a chapter to the Mature Personality, Allport's account is somewhat idealized, and the biological, psychological, social, and environmental aspects of ageing in relation to personality are largely ignored, thus eliminating from consideration a large part of the human life-span. He is quite misleading when he says, on p. 306, that '...comprehension, reasoning and judgement do not measurably decline'. He offers a more extended treatment of 'functional autonomy', and in spite of some bias in the selection of evidence there has been a real attempt to show its range of application and to meet criticisms.

Allport's preference for clinical methods of judgement over actuarial methods seems to go against all contemporary evidence, and undismayed he says (p. 390) '...psychologists have not yet learned how to discover the personal dispositions upon which to base their predictions'. And (p. 533) '...when...psychology develops its morphogenic methods...' for dealing with organized patterns and systems '...and makes proper use of them, the predictions (which at best are now weak) should improve'. But presumably when they do the actuarial predictions will also improve. He seems to misunderstand the relation between clinical and actuarial methods: it is not a question of contrasting probabilities for numbers of people with certainties for the individual case, but of contrasting the frequency with which each method can accurately forecast for individuals. No one doubts that one can make individual predictions more successfully if one knows something about the person's salient 'personal dispositions' and their interrelations; but Allport offers no original or comprehensive ideas for studying such characteristics. The concept of 'personal disposition' is Allport's solution to the problem of individual versus common traits. He does not consider the alternative linguistic solution which would regard trait-names as polymorphous, i.e. their meaning varies with the context. It is a pity that Allport has not explored the semantic and psycholexical aspects of personality more thoroughly, especially as he is a pioneer in this field. One feels that there are many interesting facts to be discovered through the analysis of vocabulary and common usage. Allport's survey of methods is excellent in its way; it is broad and representative, but he makes little or no attempt to evaluate particular methods. His account of expressive behaviour is very good, but it offers little real assistance in practical assessment, and omits all reference to clothing. The chapter on Person Perception is an excellent introduction to this topic. In spite of his criticisms, the reviewer feels that the book as a whole makes a significant contribution to psychology.

D. B. BROMLEY

Decision, Order and Time in Human Affairs. By G. L. S. SHACKLE. London: Cambridge University Press. 1961. Pp. xiv+302. 35*s*.

It is interesting to reflect that whilst we give to our important decisions careful preliminary thought and often no small worry, and then expect these decisions to influence subsequent events, yet we continue to tolerate, as adequate explanations of human behaviour, theories which imply that such decisions are empty, illusory or powerless.

It may in the simpler realms of theory be useful for us to postulate a psychological man—'a being which moves in predestinate grooves, not a trolley or bus but a tram'; or economic man who has consistent tastes, responds rationally to all circumstances, and makes his choice in the light of complete information. But before they are fully satisfactory, these schemata will have to be such that the observer will see them not only as tools, but as explanations of his own behaviour. At present we implicitly exclude ourselves, as observers, from the application of these schemata; they are descriptions of others. At the same time we cannot do without explanations not only of the behaviour of others but of ours as well; for it is the order which we impose upon our world which constitutes our understanding of it, and in a meaningless world we are powerless.

Hitherto the psychologist has not been faced with the need to find a new dynamic explanation, as he can make the occasional prediction by explaining the past as fully as possible and arguing from the past to the future. The observer can then continue his march into the future, with his back to it.

The economist is not so fortunate, for in Britain, at least, he has the unenviable role of predicting for the government human behaviour in the mass. His observer must face the future, and so the economist requires a system of calculation, a 'dynamics' which will enable him to make prophecies of the course of future history. The explanation must be adequate to describe decisions made in conditions of uncertainty, and it must be such that the observer can accept it as an explanation of his own behaviour.

It is with these problems and with this need that this book is concerned. Prof. Shackle has been engaged in this inquiry for at least fifteen years, and he now presents the latest developments in his explanation.

He starts with the formidable assumption about the nature of things, 'that the rival possible outcomes which a man will imagine for an available act of his own cannot be listed from a knowledge, however complete, of what is and what has been'. In consequence decision contains a creative element and can no longer be considered as empty, illusory or powerless. The book goes on to explain the part played in the explanation by time, decision and expectations. The 'order' of the title refers of course to the explanatory scheme.

The second part of the book studies the nature of uncertainty and whether probability in any of its accepted senses can be properly applied to decision-making. Shackle then considers the attractiveness of competing choices and develops from this a theoretical model for decision-making, and in the final part gives economic illustrations of his argument from the fields of investment, profit and interest. The final chapter sums up the purpose of the explanation and relates it to psychological explanation. 'Physical law and psychological law can both be looked upon as parts of natural law. Economic theory is an endeavour to systematize part of psychological law.' In other words, economic behaviour is as much a part of psychology as neurotic behaviour, and theories and explanations generated in the one field ought to find their use and application in the other.

The argument developed in this book requires great concentration to follow and a definite effort of understanding. This arises from the difficult nature of the subject, not from any shortcomings of the author, for on more mundane themes of economics and mathematics Prof. Shackle is a clear expositor. It is likely to be of most interest to those who are concerned with developing explanations of human behaviour, or with the problems of statistical versus clinical prediction, or with what might be called the philosophy of psychology. But any psychologist who makes the effort to understand the argument will find it novel, stimulating and enlightening.

D. C. DUNCAN

The Existence of Mind. By JOHN BELOFF. London: Macgibbon and Kee. 1962. Pp. 263. 35*s*.

The main thesis of this excellent book is that 'minds, mental entities and mental phenomena exist as ultimate constituents of the world in which we live'. Dr Beloff commences by discussing the various 'solutions' that have been devised for the 'mind–body' problem, and concludes that only two are viable today—interactionism and psycho-neural identity. Behaviourism (Rylean) is dismissed as 'methodologically misleading, philosophically false and ideologically pernicious' and phenomenalism likewise is 'powerless to sustain conviction except among those whose thresholds of paradox-tolerance is, presumably, unusually high'. This is followed by an account of perception that defends the representative theory (on Broadian lines). First due obsequies are paid at the grave of phenomenalism and then naïve realism is sturdily consigned to the wastepaper basket (particularly the howler perpetuated by Armstrong, amongst others, that hallucinations and illusions are nothing but false beliefs) by an argument compounded of physiology, double-vision, the finite velocity of light and a reminder of the circular nature of any arguments based on 'common usage'. These arguments should carry conviction to all except those rendered impervious to reason by ideology.

In dealing with the representative theory (RT) the author emphasizes some important new developments in this theory, i.e. the realization: (i) that certain mental entities are extended; (ii) that visual space is in some sense (not specified) different from, yet also shows 'a certain obvious isomorphism' with, physical space; (iii) that there are two fundamentally different logical kinds of representation (not distinguished by most critics of RT) i.e. 'mapping' (which I call 'non-symbolical') and 'coding' ('symbolical'). The primary qualities of objects are conveyed by the former and the secondary qualities by the latter. Most critics of RT have assumed that the logic of the latter applied to the whole of perception, whereas the entire problem devolves on the fact that the logic of the two forms of representation is quite different. The former, for example, is always non-inferential and this avoids the charge that RT must make the (untrue) claim that our knowledge of the external world is based on inference.

Dr Beloff then proceeds to a discussion of thought, with particular reference to cybernetics. He suggests that the theory that thinking is 'a function *exclusively* of the brain and nervous system conceived as an autonomous computor' can be criticized on three counts: (1) 'lack of plausibility with respect to the facts of creative originality; (2) inadequacy to account for meaning and intention; and (3) inability to do justice to the unspecifiable component of human thought.' Mackay has dealt rather severely with (1), and (3) depends largely on Gödel's theorem (which surely applies to computors as well?). Sections on action and personal identity suggest that the important decisions in unravelling the mind–brain problem lie in the fields covered by chapters 2 and 3 and the answers there obtained can then be used to deal with problems of action and personal identity. Finally there is a balanced account of parapsychology.

The book throughout is written in a modest, lively and witty style and makes easy reading without sacrifice of depth. It should certainly be required reading for all students of philosophy and psychology.

J. R. SMYTHIES

Animal Behaviour: Its Normal and Abnormal Development. By L. V. KRUSHINSKII. (Translated by Basil Haigh.) New York: Consultants Bureau. 1962. Pp. xviii + 261. $12.50.

The author of the book is head of the Moscow University Laboratory on the Pathophysiology of Higher Nervous Activities and, as one might expect, a notable feature of the monograph is the extent to which Krushinskii has applied Pavlovian concepts concerning the functional state of the central nervous system to account for aspects of pathological behaviour in animals. The book, which covers much of the author's published work, may be considered in terms of three main areas of research. The first part is devoted to studies of the incidence and nature of aggressive and defensive behaviour in dogs. A second section is concerned with the behavioural and physiological determinants of audiogenic seizures and, in the final part, an analysis of a type of detour problem-solving behaviour is presented, mainly with birds and rabbits as experimental subjects, and interpreted in terms of the operation of the 'extrapolation reflex'.

In describing studies of the emotional behaviour of dogs, Krushinskii's views relating to the analysis of animal behaviour in general are made clear. He contends that behavioural acts arise through a complex integration of conditioned and unconditioned reflexes that together form 'unitary reactions of behaviour'. The reflex, in keeping with the orthodox view, is regarded as the simplest integrated unit of cortical function and the unitary reaction is defined as the basic unit of overt behaviour. A third step is to propose that 'biological forms of behaviour' (for example, feeding, sexual, and defensive activities) are built up from individual unitary reactions, and it is suggested that only by studying the degree of inheritance of behavioural acts is it possible to proceed to a study of their dependence on individual experience. To students of comparative psychology and ethology an hierarchical scheme of this kind is not particularly new, although in so far as it enables Krushinskii to extend concepts derived from classical studies of conditioning to other aspects of animal behaviour, it appears to be a promising development.

During the Second World War, Krushinskii served as a scientific advisor on the training of dogs for military work and the first three chapters are devoted mainly to his studies of the genetic and entogenetic determinants of the active defensive reaction (ADR) and the passive defensive reaction (PDR). Although precise definitions of these terms are not given, the former refers to aggressive behaviour while the latter refers to defensive, withdrawal responses. The incidence of the two patterns of behaviour has been studied extensively in given dog breeds as well as in crosses made between breeds and the results of the experiments are depicted by means of genealogical tables. Apart from the genetic studies, many experiments describing the effects of various drugs on the reactions have been made and it is of interest to note that morphine has been found to dissociate the two forms of defensive behaviour: the drug suppresses the active defensive reaction in 'aggressive' dogs but appears not to affect the passive defensive reaction of 'cowardly' animals. The interpretation of these experiments concerning individual differences in emotional reactivity is made in terms of the two conceptual properties of the central nervous system originally described by Pavlov, namely, its 'strength' and its degree of 'excitability'.

The most rigorous application of Pavlovian concepts is to be found, however, in Krushinskii's study of convulsive behaviour in rats and mice and it is clear that the experiments are consistent to a considerable degree with predictions derived from classical theory. By varying the intensity and duration of auditory stimuli in the production of seizures, rapid changes in motor activity are observed and interpreted in terms of the opposition of central excitatory and inhibitory processes. As explanatory devices, such concepts as the ultraparadoxical phase and limiting inhibition seem to be well suited to account for Krushinskii's observations. Many experiments are also described in which drugs have been used to affect the excitability of the central nervous system as measured indirectly by changing susceptibility to seizure. This area of Krushinskii's work provides much new information and is likely to revive psychological interest in the problem, especially in view of recent doubts that audiogenic seizures are attributable to infection of the middle-ear (otitis media).

In a final section of the book, a number of studies of problem-solving behaviour in pigeons, crows, domestic fowl, and rabbits are described. The basic scheme of the experiments is as follows: food, in a bowl, is moved in a straight line and at constant speed in full view of the animal; after a short period, it is hidden beneath a cover. Observations are then made to determine whether the subject continues to search for the food and whether the animal is able 'to extrapolate the direction of its movement'. Screens are then introduced in various positions and the abortive responses of subjects in circumventing them are described in terms of the extent to which the animal is fearful of the situation. It is clear that these observations are preliminary ones; the phenomena under consideration must be regarded as complex and there can be little doubt that Krushinskii's use of the term 'extrapolation reflex' is hardly adequate to account for these forms of behaviour.

We are fortunate to have this translation of Krushinskii's book since many of the experimental findings will be new to students of animal behaviour in Britain and elsewhere; this is especially true as regards the genetic studies, the endocrinological findings, and Krushinskii's work with drugs. If the book is to be criticized in general terms, it is on two counts. First, the author appears reluctant to formulate exact definitions of some of the phenomena under consideration and, secondly, the inferential nature of concepts relating to cerebral function, based on behavioural rather than neurophysiological observations, is perhaps overlooked. ROGER STRETCH

Activation and Behavior. By ELIZABETH DUFFY. New York and London: Wiley. 1962. Pp. xviii + 384. 60*s*.

In 1941 Prof. Duffy published a paper in the *Journal of General Psychology* in which there was a fairly adequate statement of the concept today known as activation or arousal. This, together with the work of G. L. Freeman at about the same time, marked the conception of activation theory which eventually gained fuller recognition following the discoveries of Moruzzi and Magoun in 1949 and the publication of Lindsley's influential chapter on 'Emotion' in Stevens' *Handbook of Experimental Psychology* in 1951.

Today the idea of a vaguely defined activation or 'behavioural intensity' dimension is widely accepted and it is surprising to find that Duffy spends some time in justifying the need for such a concept. But this is a minor point. In the main the book serves a very necessary purpose in that it explores, at least to some extent, the possibilities and consequences of a developed activation theory.

The book is in three parts which are concerned with, respectively, the nature of activation, the effects of variations in activation, and individual differences in activation. Each section summarizes and attempts to integrate work drawn from a wide variety of sources. The breadth of material covered emphasizes Duffy's intention which is to show that activation theory makes many of the conventional psychological categories redundant.

In three aspects the book is less than adequate.

(*a*) It is concerned primarily with a unitary dimension called activation, and yet glosses over the hard fact that the various physiological changes which are supposed to measure this single dimension show remarkably low inter-correlations. To state, as Duffy does, that a combination of all or some of these measures may be the best indicator of activation level is hardly satisfactory when no experimental evidence is cited to prove that this is true.

(*b*) The strong implicit emphasis upon a single dimension leads to an uneasy vagueness in certain sections. Contradictory evidence is presented and then apparently forgotten in some general summary. One sometimes feels that the complexities of the problem are being disregarded by the adoption of a theoretical position which is so vague and general that anything can be forced to fit it. This tendency, if carried too far, could lead to the undoubtedly useful concept of activation becoming a truism with no scientific value.

(*c*) The challenge presented by contemporary approaches to motivation and drive has been almost completely ignored. No mention has been made of the recent work of Berlyne and Bindra and yet this would seem to be a field where a carefully developed activation theory could resolve some of the dilemmas presented by the largely linguistic classifications of the past.

The most interesting part of the book is the final section on individual differences. By emphasizing the fact that differences in activation can be measured by electrophysiological means, and that these differences appear to relate to dimensions such as 'anxiety' and 'neuroticism', as measured by verbal tests, this section holds out the hope that the psychologist may eventually have more adequate and objective means of assessing these, and possibly other, variables. But much more work on methods of physiological measurement, together with a careful consideration of the possible complexities of activation, are necessary before this becomes more than a hope.

This book is recommended to those who require an adequate summary of work relevant to this now established field, together with an indication of the possibilities presented by activation theory.

R. S. CORTEEN

Psychological Differentiation. Studies of Development. By H. A. WITKIN AND OTHERS. New York and London: Wiley. 1962. Pp. xii+418. 60*s*.

Possibly because it promises more precise and quantifiable methods of penetrating the complex field of human personality, the study of cognitive and, as in the present investigation, perceptual styles, seems to increase in popularity. *Psychological differentiation* is the second instalment of what looks like being a series. In it Witkin and his associates have extended to a scrutiny of the child's developing personality their theory, already outlined in *Personality through perception*, that mode of field approach (whether field-dependent or field-independent) is associated with many other facets of an individual's behaviour. Mode of field approach they define in terms of performance on the rod-and-frame test, one of the tilting-room-tilting-chair tests, and their embedded figures test. Thus it had been found in earlier work that field-dependence is associated with conformity to other people's opinions, field-independence with forming one's own judgements, that field-dependent subjects are more suggestible than field-independent subjects, and that girls and women are more conforming as well as more field-dependent than men and boys.

The main study reported in this book has been cross-sectional, and carried out with sixty-eight 10-year-old boys, living in Brooklyn, New York, fifty-nine of them Jewish. Many of the findings have been cross-validated. Longitudinal studies of another 185 boys and girls were only partly completed at the time of publication.

The present report is concerned largely with these children. However, a great deal of research conducted by many people over the last 20 years on mode of organizing the perceptual field and linking it with other aspects of behaviour has been summarized, as well as much of the author's previous work. Evidence has accumulated suggesting that field dependence-independence may have wide generality and stability through life. There is no evidence that it is genetically determined, no studies yet having been reported of identical twins reared apart. At least two pairs of such twins, reared together, have been studied, who in each case were distinctly different in their perceptual test results. Children apparently progress from less to more field-independence up to about the age of 17, although their perceptual style seems to be established fairly early in life, at about 8–10 years. The type of relationship the mother has with her boy is positively and significantly related to the latter's field approach.

Studies looking for general and stable personality characteristics are naturally of interest to the clinician. Anyone who might have hoped that, with the installation of a tilting-room-and-tilting-chair, or even a pack of embedded figures, his psycho-diagnostic problems might all be solved, will be disappointed. Mode of field approach is not related to either presence or absence of healthy psychological functioning. Patients diagnosed as suffering from schizophrenia 'have perceptual performances ranging along the whole field-dependence-independence continuum'. On the other hand, patients of differing diagnoses but all suffering from ulcers have been found to be relatively field-dependent as defined by the rod-and-frame test; hysterics appear more field-dependent on the embedded figures test than obsessive-compulsive patients; psychotics with hallucinations are apparently likely to be more field-dependent than psychotics with delusions, again using the embedded figures test; alcoholics and obese people tend to be 'extremely field-dependent'; while hospitalized male paranoid patients are reported 'to be perceptually field-independent'.

This book could have been shorter. Nevertheless, it merits close study, both for its general theoretical implications, and also for its methodological contributions to experimental child psychology.

J. GRAHAM WHITE

Values in Psychotherapy. By CHARLOTTE BUHLER. New York: Free Press of Glencoe. 1962. Pp. xx + 251. $5.25.

Group Psychotherapy. By HUGH MULLAN and MAX ROSENBAUM. New York: Free Press of Glencoe. 1962. Pp. xviii + 360. $5.95.

Recently the psychiatrist and psychotherapist have been challenged on the grounds that they are dealing with moral issues as much as with medical problems. Charlotte Buhler's book, although it does not appear to have been stimulated by this trend, is nevertheless well-timed. She presents the problem of values in the setting of psychotherapy in such a way that the issues can be generalized for the psychology of values and even for moral philosophy. It is surprising that psycho-analysis has ducked the issue of values for so long in spite of the grumblings of diehard moralists. The reasons for this are interesting. Buhler suggests that psychotherapy's previous concern with values only in so far as they revealed the patient's neurotic drive was a reflexion of the 'passive' phase of psycho-analysis. The therapist in his passive role aimed to be amoral. The current trend is a return to the more active pedagogic procedures advocated by the early Freud and to an examination of value-systems in the patient–therapist relationship. In this phase the holistic approach is taken to be an oversimplification, the criterion of 'adjustment to society' is false, and the goal of treatment is 'self realization' with stress on creativity and accomplishment. Add to this the contemporary tendency in ego-psychology to stress conscious rather than unconscious processes and the modern psychoanalysis is a vastly different system from the confused 'neo-Freudian' version of the immediate post-war era, with voluntarist tendencies rather than the hackneyed psychic determinism.

Characteristically Buhler sets values at their point of origin in the infant's 'trust' and their development is traced in a tabular chronology to maturity and postmaturity.

While this book will not allow the psychologist to declare the moral philosopher redundant it is provocative and stimulating and could be read by many who are not primarily concerned with psychotherapy.

Mullan's and Rosenbaum's volume is different in that it is a primer of group psychotherapy for psychiatrists, psychologists and social workers who do this kind of thing. Although theoretical issues are raised in a half-hearted fashion, the authors seem happiest and most enthusiastic in telling the reader how to select and prepare the patient, conduct and exploit the contact with the patients and deal with situations (including the death of the therapist and the arrangement of fees). The book is too American to be of more than interest for the British reader. The group is valued because it is a microcosm of democracy 'à l'American' and the touchy problem of how effective is the treatment is not even hinted at.

R. M. MOWBRAY

The Year Book of Education, 1962. *'The Gifted Child'*. Edited by G. Z. F. Bereday and J. A. Lauwerys. London: Evans. 1962. Pp. xiv + 541. 63*s*.

The Year Book of Education can usually be relied upon to deal with an aspect of education which is 'in the news', and this year's volume is no exception. The Western world is particularly concerned at present with the inescapable necessity of doing all it can to discover and nurture talent. It sees this not only as an essential weapon in the cold war, but also as the most powerful aid to the faster development of the developing countries. There seems no doubt, then, of the success of this publication on the grounds of topicality. And the authoritative nature of much of its contents will make it welcome to a wide readership of educationists and psychologists.

Following a brilliant and substantial introduction by Sir Cyril Burt, the book is divided into three sections: Psychological Theories; Detection of Ability and Selection for Educational Purposes; and Increasing the Pool of Talent. With forty-one contributors from nineteen different countries we get not only a rich diversity of approach but also an inevitable unevenness in quality. The trend, observable over the past few years, of reducing the number of contributors, ought to be continued—and might one suggest that the editors be rather more ruthless in wielding the blue pencil?

The most valuable part of the book, in the reviewer's judgement, is Burt's general introduction. Indeed this, together with his recent critical notice of Getzels and Jackson's *Creativity and Intelligence* (*Brit. J. Educ. Psychol.* 1962, **32**, 292–8), provides the student with a most comprehensive review of the most significant work on the nature of talent. Among other things, it puts into perspective the recent (and highly fashionable) American work on 'creativity' which has developed from Guilford's 1959 paper, and admirably orientates the reader before tackling the diverse viewpoints presented in section I. This section includes the Freudian viewpoint presented by Greenacre who, among other things, detects a correlation between lightning calculation and masturbation. Henderson re-hashes an address given by Jung in 1942 on 'The Gifted Child', a paper which opposes special schools or classes for academically gifted children (on the grounds that 'this would lead to breeding in them an undue and unreal sense of their importance') and yet advocates 'the detection and encouragement of children with gifted hearts'! The classroom teacher will find little practical advice on either the selection or treatment of this category. Guilford provides a chapter 'Parameters and Categories of Talent' which exemplifies the American approach to the categorization of abilities. This shows that 'there are 120 ways in which to be talented', and that the psychologist has at his disposal 'fiftyfive known factors of intelligence'. Torrance's chapter gives a useful survey of recent American work on giftedness and creative ability, with a four-page bibliography.

Section II, on the detection of talent, gives us seventeen chapters from many different countries. Unfortunately, one or two contributors content themselves with general essays on problems of selection, without giving us much information about their own problems or methods. There are wide differences in approach as well as in levels of sophistication. If a content analysis were performed on these chapters, factorization might produce some interesting results: a pleonastic factor (Mallet on France), for example; sciolistic (Roger on Belgium); complacent (Winterbourn on New Zealand); or a straightforward *m'aidez* (Gómez on Venezuela). There are excellent contributions—as might be expected—from Husén of Sweden and Biesheuvel of South Africa.

Section III, on increasing the pool, has a short but pertinent chapter by Havighurst, a rather evangelical contribution by Fleming, and an even more emotional one by Drews—who has, however, some interesting things to say about method in the classroom. Curle sees community development as a universal panacea in the under-developed countries, while Bierfelder applies typology to education for management. The best chapters here are by McIntosh of Scotland, Spitz of Holland and Ginzberg of Columbia. C. A. Anderson writes a stimulating chapter from the point of view of the sociologist, taking Young's *Rise of the Meritocracy* as a text. He sees counselling sliding over into manipulation, the multiplication of external examinations, the distortion of educational and vocational choice, and an enhanced prestige of subject-oriented teachers over pupil-oriented teachers. He writes attractively, but with exaggeration. Nevertheless, the warning he sounds is necessary.

The major criticism the reviewer has of this large volume is the lack of emphasis on information and advice on how to educate the gifted child once he has been discovered. We have the usual mention of 'acceleration' and 'enrichment', but the teacher, the headmaster or the education officer will find little discussion of classroom problems and school methods. The major problem with the really gifted is to enable them fully to achieve their potentialities. There is probably no other section of the school population which falls so far below the ceiling of achievement. The problems in the school are many, not the least being the inevitability of the teachers often being of a lower intellectual calibre than their pupils. To this may be added the cramping effect of rigid time-tables, limited text-books, circumscribed syllabuses and the subject-limitations imposed by examination requirements. Ideally, the gifted child ought to be stimulated to exercise his talents as fully as possible, in an environment which provides all the material he may need to do this. In practice, if one can prevent the school from unduly handicapping him in his academic activities, from stifling emergent interests, from blocking the development of his intellectual skills, then this is about as much as we can hope for. Perhaps not one in ten of the top 2 % ever gets even this limited chance. Fortunately, the very able are spared the confusion which poor teaching can produce in the mind of the average and below-average child. If we remove the major obstacles to their wide-ranging minds, and provide opportunities for experiment, for inquiry and for research, then we may see at last a partial flowering of their exceptional gifts.

STEPHEN WISEMAN

The Widening World of Childhood: Paths towards Mastery. By LOIS BARCLAY MURPHY and Associates. New York: Basic Books. 1962. Pp. xvi+399. $10.00.

The special interest of this book lies in the fact that it is concerned with 'normal' children and the resources which they 'mobilized and the methods they developed in their efforts to cope with life'. It is based on more than twelve years study by a group of specialists of thirty-two children who had previously been observed during infancy and who were further intensively studied at the pre-school phase. They represent a sample of the middle-class white majority in a middle-sized town in the Middle West of America. Each child was seen in a dozen or more different sessions, in his home, in intelligence and personality test situations, in play situations, in structured group situations, e.g. at a party and in dealing with difficulties or stress situations.

'Coping patterns are most easily seen when a child or an adult is confronting a new situation which cannot be handled by reflex, habitual, or other routine or automatic action. In this sense, the study of coping puts the emphasis on the process of developing ways of dealing with new and difficult situations. Once such methods are consolidated, competence or mastery may result' (p. 6). In these terms Dr Murphy defines 'the concept of coping'.

The very comprehensive records of the children's response to adults and children in novel situations, the ways by which they come to terms with the challenges presented to them are original and illuminating. They illustrate the richness, the variety and the highly individual manner in which the pre-school child copes with his environment and his growing mastery of his powers and development of independence. The style at times is refreshing and almost lyrical. In describing the differing reactions of children to new experiences the author writes 'For still another (child) strangeness is simply a question mark, something to discover; this child will let strangeness have a fair chance. He allows it to show its colours; he does not prejudge it. One child may march forward, ready to beard the lion in his den if need be, while another skips into newness as if it carried a rainbow's promise of a pot of gold' (p. 193).

The section of the book which describes in considerable detail the ways by which a child of three dealt with the experience of a severe accident and the necessary hospital treatment, and how a five-year-old coped with the disabling after-effects of polio two years earlier are of particular interest. The comparative studies of these thirty-two children at the pre-school stage with their behaviour recorded in their first months of life tend to confirm the hypothesis that the basic patterning of the child's relation with his environment and of 'the coping capacity of the ego' is laid down in the first six months of life, the stage of oral gratification. Correlations of the ratings of behaviour in many different aspects both in infancy and during the pre-school period are listed in the Appendix.

Some of the themes are rather laboured and to the British reader the theoretical discussion is at times tedious and verbose, but the book makes an important contribution to the literature concerning normal child development. Further publications based on the findings of this research are to be expected in the near future.

AGATHA H. BOWLEY

Delinquency and Child Neglect. By Harriett Wilson. London: Allen and Unwin. Pp. 195. 25*s*.

The author—a social worker—has studied fifty-two 'problem families' with the two-fold intention of investigating their general backgrounds and ascertaining whether an undue proportion of delinquent children come from these homes. Although she has not applied the standards of rigour to which most psychologists are accustomed in connexion with experimental and research designs, the author gives valuable insight into some of the investigated cases. It is always difficult, of course, to draw general inferences from the particulars of unmatched cases. Nevertheless, her findings tend to make a great deal of sense even though a more psychometrically tight study on this topic has yet to be undertaken in this country. The author clearly understands the complexities of the variant backgrounds of these families, knows her literature on the subject, and in addition to her findings, makes some suggestions for changes in the law, designed to benefit both 'problem families' and society at large.

The book falls into three unequal sections. The first two chapters deal respectively with the literature on this subject—mainly English—and with comments about the aims and methods of the survey. It is clear that a socio-clinical approach dominates the statistical throughout. The next section comprises three chapters dealing with the broad results of investigations into the backgrounds of these families in terms of their appalling housing and financial conditions, the general inadequacy of the parents, with emphasis on intelligence, harmony, and their capacity for handling their children imaginatively.

The second section consists of two chapters specifically on delinquency. The author had planned to dichotomize the problem families into those who had delinquent children and those who had not, and to compare significant results with inter-correlated items concerning the individual and socio-economic background factors. However, inspection of the figures showed that it was not possible to divide the total sample into two groups because practically all families had not only one but often several delinquent children. This led the author to compare her findings with those of a similar study made by Grünhut in 1956. The results showed that the delinquency rate among families exhibiting child-neglect was eight times as high as the general rate for boys in the whole city and double that of their immediate neighbours. A few case-histories are given which show that in addition to general substandard inadequacies, these families show clusterings of pathology, some having a preponderence of inadequate and constantly child-bearing mothers, others inadequate and chronically underemployed fathers, and others very poor housing conditions.

The third section, consisting of the final chapter, makes interesting reading in so far as some practical suggestions are offered, mainly regarding possible legal changes. For instance it is suggested that family allowances should be progressively raised with the arrival of each new child to provide already poverty-stricken families with the semblance of a subsistence level. In order to help other cases, it is suggested that it should be possible to provide lump sums where and when necessary to be used during specific emergencies, to extend family planning clinics and to provide nursery schools for the two- to five-year olds. Above all, a plea is made for extending and integrating the work of the excellent Family Service Units.

It is clear that this book will interest practitioners more than academics. However, this small volume plays a useful part in temporarily filling a gap which exists in this field of study.

R. G. ANDRY

Educating Older People. By M. F. Cleugh. London: Tavistock. 1962. Pp. 185. 28*s*.

Almost all our systematic knowledge about the educational and learning processes is confined to the first 20–25 years of life. Any contribution to understanding what may happen among people older than this is consequently seized upon eagerly by those few psychologists who have become interested in that neglected subject—the normal human adult. Mr Cleugh bases his observations on university students in the age-range 27–51 years: these are men and women taking advanced courses designed for practising school-teachers. While this means they are a rather select group, much of what the author has to say will find an echo in the experience of WEA and extra-mural tutors. Since many psychologists are likely to find themselves involved in these fields as well as in that described by Mr Cleugh, it is a pleasure to be able to recommend this book for its cogency, lucidity and humanity. Those who teach younger adults might also gain much from studying it with a proper humility.

ALASTAIR HERON

Reading Disability: Progress and Research Needs in Dyslexia. Edited by JOHN MONEY. Baltimore: Johns Hopkins Press; London: University Press. Pp. x + 222. 40*s*.

This volume contains papers given at the Johns Hopkins Conference on Dyslexia and Related Aphasic Disorders and contains reports of some of the discussions which followed them. The scope of the Conference was wide, as indicated by the fact that papers were given about psychiatric, educational, causal and clinical aspects of dyslexia. All but two of the speakers, Prof. Zangwill of Cambridge and Dr Heinz Prechtl of Groningen, were American.

Any reader who wishes to know what was said of importance can hardly do better than read the first chapter by John Money entitled, 'Dyslexia: a Postconference Review'. This will probably tell him all he needs to know.

If he reads the other contributions in more detail he will find rather little that is new or exciting except possibly in the chapter by David Howes on 'An Approach to the Quantitative Analysis of Word Blindness', which merits attention. Zangwill's paper on 'Dyslexia in Relation to Cerebral Dominance' is an illuminating review of current views on the subject and Gilbert Schiffman discusses recognition and treatment of dyslexia in a thorough and realistic manner in his chapter on 'Dyslexia as an Educational Phenomenon: Its Recognition and Treatment'. On the other hand, Saunders on 'The Phenomenology' of dyslexia, Hardy on 'Dyslexia in Relation to Diagnostic Methodology in Hearing and Speech Disorders' and Wepman on 'The Relationship of Dyslexia to Language Acquisition and Concept Formation' are disappointing. They give no clear account of dyslexia as a disturbance of language.

The book is well produced, the print is clear, there is a useful glossary, a twelve-page bibliography with a rather strong American bias, as might have been expected, and adequate indexes.

T. T. S. INGRAM

The Science of Dreams. By EDWIN DIAMOND. London: Eyre and Spottiswoode. 1962. Pp. x + 246. 21*s*.

This book stems from an earlier *Newsweek* cover story by the author, a journalist, who employs the dramatic sensational style frequently associated with popular scientific writing.

Recent electrophysiological studies, which originated in Chicago ten years ago, have revealed the regular accompaniment of dreaming by recordable bodily events, notably rapid eye movements and characteristic electro-encephalographic activity, which are now used as the indices to dreaming. These studies form the core of Mr Diamond's book and he excites our imagination with an enthusiastic account of up-to-the-minute reports of unpublished 'findings', research in embryo and plans for future research, supported by numerous excursions into dream folk-lore, including a fascinating description of the dream cult of the Iroquois Indians.

It is regrettable that despite considerable effort to provide one with a complete picture of the rapidly expanding output from numerous dream laboratories in America, Mr Diamond makes no mention of work carried out elsewhere, particularly in France and in Britain. Furthermore, Mr Diamond becomes so transported by the new vistas opened up by the utilization of electronic techniques in dream research, that he fails to display a critical attitude to numerous tentative interpretations of unconfirmed experimental results, especially when he considers the relationship between the rapid eye movements and recalled dream imagery, and expositions of a 'need to dream'.

Although this book will no doubt have popular appeal, it cannot be recommended to the scientist, whose requirements would be better satisfied by Oswald's *Sleeping and Waking*, which provides a more accurate, comprehensive and critical review of the field.

RALPH BERGER

Problems in Addiction: Alcohol and Drug Addiction. Edited by WILLIAM C. BIER, S.J. New York: Fordham University Press. 1962. Pp. xii + 247. $5.00.

This book comprises an edited series of papers read at the 1959 Institute in Pastoral Psychology, a Conference sponsored by the Psychology Department of Fordham University. The aim of these institutes is to help clergymen to understand emotional problems, the relationships and values of different approaches to these problems and how to cope effectively with them. In 1959 the problems of alcohol and drug addiction were considered. This book then is not so much a body of knowledge as a series of stimuli presumably intended to reach those prevented from attending the Conference. It seems to have been a good conference. The papers read well, and sound convincing, even to the non-believer. One may identify with the speakers or one may feel stimulated to try harder in future. One feels eager to contact, to understand and generally to help the alcoholic, and even the drug addict does not seem so hopeless. The papers deal fairly and squarely with problems of guilt and responsibility, and with scientific doubts regarding etiology and classification. Just occasionally did speakers seem to be discharging their own tensions rather than dealing with their audiences.

Any book of collected papers is likely to be uneven. I liked Dr Robert J. Campbell's balanced account of the Etiology of Alcoholism and Dr Ruth Fox's descriptions of psychodynamics within the alcoholic's family. Father John Ford, S.J. in his account of Pastoral Treatment reminds me of an experienced clinical teacher conveying not so much hard facts but, by example, the ability to remain intellectually imperturbable. I am sure that (from his standpoint) everything he says is true; all one now wishes for is the opportunity to learn how to apply it. The chapters in praise of Alcoholics Anonymous seemed less necessary, but perhaps there still exist people who do not realize A.A. is the best thing so far that has happened to alcoholics. The section on drug addiction deals mainly with problems that scarcely arise in this country.

This is a good book of its kind. It is easy to read and should be stimulating to the Priest or lay-therapist who, like the rest of us, at times cannot see the wood for the trees.

J. AMOR ARDIS

Psychology of Survival: Human Reactions to the Catastrophes of War. By W. VON GREYERZ. Amsterdam and New York: Elsevier. 1962. Pp. x+99. Dfl. 10.

An Alternative to War or Surrender. By C. E. OSGOOD. Urbana, Ill.: University of Illinois Press. 1962. Pp. vii+183. $1.45.

These books seem nearly complementary. The first assesses some psychological problems encountered, so far, during catastrophes in war, and techniques to prepare for them in advance. Unlike some familiar breezy civil defence exhortations, it reports objectively experiences of victims, e.g. of Hiroshima. This psychological advice is given in a tight-lipped prose—or so the excellent translation suggests—and, in the prevailing fashion, the author has little time—in both senses—for any individual person. Nor, of course, has the H-bomb, which will annihilate a whole city, and maim thousands on its fringe, suddenly and impartially smudging out doctors and nurses, who in other wars, while undergoing hardships and dangers, were often protected because of their usefulness. In this book there seems no awareness of the four-minute warning, of the added danger as separate nations acquire the H-bomb for prestige reasons, or of the possibility, mentioned by Osgood, that the last and final war might be over in forty-eight hours. Though the author has studied the methods of civil defence in eleven countries (apparently not in the USSR) and worked in a hospital for Finnish war invalids, it is only fair to note that his book is written primarily for the Swedes. Occasionally the attractive short sentences suggest naïveté; e.g. '...it is wise to determine what possibilities of defense our country and we ourselves have. Knowledge of the structure of the military defense is necessary.'

The second book is by the president of the American Psychological Association. His treatment is also objective, but not leisured or calm. He knows well the attractions and seductions of all ivory laboratories: 'So Little Time' is its keynote. He knows well that any reliable announcement of a sudden peace would panic the Stock Exchanges, threaten unemployment, not only in the war factories, and revive the 'dread of the bowler hat' (J. B. Priestley's phrase). Many a physicist would be deprived of his present method of earning his living...these, and not writers like himself, he calls the idealists, 'who honestly believe that stabilized deterrence can be continued indefinitely while they go on playing with their nuclear toys, along with the generals who honestly believe that the men behind the weapons are completely rational and will behave like so many computers'.

Osgood's proposals are really psychological (War does begin in the minds of men) not refracted through a medically oriented mind. The plan, which he calls 'Graduated Reciprocation in Tension-Reduction'—GRIT) assumes that the problems we face are primarily matters of human nature and human relations, and that the Soviet people and their leaders are more like us than the 'bogey man' 'our 'psycho-logic' creates. His programme would put the arms race in reverse. From time to time the United States would initiate steps designed to reduce world tensions. These steps would be announced prior to their execution and on the dates set *these steps would be taken*. They would increase in significance if and as they are matched by actions of other nations. 'Together we can move, step by careful step, back away from the abyss that threatens to engulf us all', and the first step would be to make public all medical information the USA have been gathering concerning man-in-space.

My tachistoscopic view of Osgood's programme may be misleading. It may cause the reader to object 'But I've heard this before'. He has not. I hope that a British publisher will soon help him to realize this.

T. H. PEAR

Studies in Individual Differences. The Search for Intelligence. By JAMES J. JENKINS and DONALD G. PATERSON (eds.). New York: Appleton-Century-Crofts; London: Methuen. 1961. Pp. xvi + 774. 63*s*.

This is a collection of sixty-six original research articles, arranged in historical sequence from Sir Francis Galton (1896) to Guilford (1959), and designed to acquaint the student with some of the 'initial inspirations, false starts, first approximations, and attained half-truths' upon which current text-book knowledge is based. This it does, and admirably so. Whether the more ambitious aim of fostering 'good psychological judgement, an elusive quality called perspective, and a grasp of the strategies of psychological measurement' is fulfilled is less certain, and will largely depend on the quality of the individual student.

The chronological arrangement brings into prominence the comparative absence of change, over the years, in the specific issues considered important to the study of 'intelligence', in contrast to the marked changes in the methodology of their investigation. Many issues (racial and social class differences in I.Q.; heredity and environment) recur throughout, and are still controversial, and it is quite apparent that the 'search for intelligence' is still far from a satisfactory conclusion. The only major differences in content between the later articles and those of earlier date is the abandonment of attempts to measure intelligence via simple sensory and motor tasks (e.g. Wissler, 1901), and the ever-increasing emphasis on interrelationships between intelligence and temperament and personality (e.g. Wechsler, 1950). But it is from the point of view of methodology that the book makes perhaps its greatest impact, many articles having deliberately been chosen for their lucid exposition of a particular experimental method—e.g. Woodworth's 'Heredity and Environment: Twins' (1940), which may even imbue the reader with a renewed interest in the hackneyed nature–nurture problem.

The sixty-six articles are not claimed as the 'best' available—merely as being valuable, thought-provoking, and representative. Their value is, inevitably, uneven, but all have their points of interest. The inclusion of lesser known authors, as well as the 'big names' adds to the interest and usefulness of the collection. As for representativeness, objections can always be raised in the face of a claim such as this. There are no articles on research using animal subjects (e.g. Tryon), and almost nothing on biologically or neurologically oriented research. Nevertheless, the survey of topics is adequately broad. The sampling of authors, on the other hand, is restrictively narrow. There is an over-abundance of articles of American origin, and one wearies of reports on immigrant groups, Negroes, and college freshmen. Articles by British psychologists peter out after Spearman (1927), and, apart from Husén (1951), the Continent is scarcely represented at all. This is not a true reflexion of the state of British and European research.

The major weaknesses of the book lie in its structure. There is no index of subjects, nor even an alphabetical list of authors' names. While the chronological arrangement of articles has its merits, a grouping together of all the articles on any one topic might have been more convenient, with chronological arrangement within each group. The tone of the brief introductory comments to the individual articles tends to be a little patronizing, with too free use of 'of course', 'without question', etc., and some readers will be less than fully content with the summaries of omitted portions of many articles.

ALISON CAMPBELL

Documents of Gestalt Psychology. Edited by Mary Henle. Berkeley and Los Angeles: University of California Press; London: Cambridge University Press. 1961. Pp. xii + 352. 64*s.*

The present volume almost inevitably invites comparison with the *Source Book of Gestalt Psychology* edited by W. D. Ellis and first published in 1938. Ellis's volume contained a selection of extracts from the key papers of the Gestalt school of psychologists published between 1915 and 1929. As all the original papers were in German and not always easy to obtain, the *Source Book* became an invaluable work of reference for English-speaking psychologists.

Mary Henle's collection is in a sense a continuation of Ellis's. It contains twenty-two papers published between 1934 and 1960, by Köhler, Wertheimer, Wallach, Asch, Arnheim, Henle and others, and is divided into five parts: (i) a series of semi-psychological papers by Wertheimer; (ii) general theory; (iii) cognitive processes; (iv) social psychology and motivation; (v) psychology of expression and art.

For the student of psychology it is a useful but hardly, like Ellis's, an indispensable anthology. All the papers but one are available already in English, mostly in easily accessible journals. The principle upon which the selection of papers has been made is not quite clear. Some of the papers are relatively slight, while major topics like figural after-effects have been virtually ignored. Nor has Gestalt psychology been marked in the period since its migration from Germany by quite the same confident brilliance.

L. S. HEARNSHAW

New Directions in Psychology. By R. Brown, E. Galanter, E. H. Hess and G. Mandler. New York: Holt, Rinehart and Winston. 1962. Pp. x + 353. 12*s.*

This paper-back aims, in the words of T. M. Newcomb's foreword, at presenting 'to the student or layman an account, fresh and informed but not highly technical, of recent contributions to a few problem areas'. It performs its task remarkably well, and should also interest many established psychologists who find it difficult to 'keep up' in all the numerous areas in which they have a general, if not a specialized, interest.

Roger Brown of M.I.T. writes on 'Models of Attitude Change' suggesting that with the theory of 'congruity-consonance-balance' some sort of compelling general pattern is at last beginning to emerge in the field of social psychology. Eugene Galanter of the University of Pennsylvania contributes a brilliant chapter of 'Contemporary Psychophysics' which can be recommended to any student who still feels that this is a 'dry-as-dust' area of psychology. Eckhard Hess of the University of Chicago gives a broad survey of the history, basic concepts, contemporary research and future prospects of Ethology—again an excellent and most useful chapter. Finally, George Mandler of the University of Toronto writes on 'Emotion', discussing the part played by environmental events, physiological response, and prior experience in determining emotional events in the light of recent experiments by Schachter and others. The existentialist approach is decisively rejected. All the chapters are highly contemporary and there are extensive bibliographies.

L. S. HEARNSHAW

C. A. Mace. A symposium edited by Vida Carver. London: Methuen. 1962. Pp. 126. 21*s.*

The Psychology of Study. By C. A. Mace. Harmondsworth: Penguin Books—Pelican series. 1962. Pp. 120. 3*s.* 6*d.*

There is melancholy in the air over the reviewing of a volume recording the retirement of a colleague. In the case of C. A. Mace however there is compensation, as readers of the first of these two volumes will quickly find. After a pleasant introduction from Vida Carver—and who knows a man better than his secretary?—there come in succession a characteristically urbane note on C. A. Mace by Rex Knight, an account of 'The Years at St Andrews' by E. Pratt Yule, Pearl King's paper on 'Mace's Contribution to Industrial Psychology', R. S. Peters's piece about his clear-headed views on the 'Philosophy of Mind,' S. G. Lee on his 'Optimism and Constructiveness'. These all embody compliments, and well-deserved ones, for a man who has been president of both the British Psychological Society and the Aristotelian Society, and has examined in both philosophy and psychology at Oxford and Cambridge, and in many other universities in this country. Perhaps the finest compliment in the Symposium is Prof. Wisdom's full length paper forming Part 2 of the volume. Here one finds resurrected (is this unkind?) that straw man of one's Greats days, THE PLAIN MAN, and perhaps resurrected as well the still important problem of the relations between sensations and, for example, visible objects. Prof. Wisdom is to be thanked for this important piece of analysis.

The PLAIN MAN of course has his own special significance here since it might, with all respect, be said that Prof. Mace approaches in psychology (and in philosophy too?) what one used to think of when one spoke of *the plain man*, though there must be added to that hypothetical personage an intuitive grasp of the problem which in those days was in nobody's mind but which is characteristic of Mace. For throughout the book one finds phrases like 'Natural Gentleman', his '...*loving* students' (the italics are the reviewer's), 'most fairminded man', 'shrewd', 'humane', 'wisdom', 'kindness', 'honesty'. They *are* indeed descriptive. And something like this composite was in our minds when we were asked by Prichard or by Cook Wilson: 'What would the plain man think?'

Psychologists will join hands in sending off to fresh fields and pastures new C. A. Mace and his wife, and sending them, if one may say it, with thanks and love.

Mace's Pelican, *The Psychology of Study*, is characteristic. Full of common sense, with unexpectedly perceptive advice and subtle deflation of the pretentious, it could have been written by nobody else. It should be read by members of all top forms at school, and many a beginning undergraduate would benefit by its sane and practical counsel.

G. HUMPHREY

Cumulative Record (Enlarged edition). By B. F. SKINNER. New York: Appleton-Century-Crofts. 1959/61. London: Methuen. 1962. Pp. x + 426 + 52. 50*s*.

Few psychologists have been more audacious in their advocacy of experimental method in relation to all aspects of human behaviour than Prof. Skinner. This book, which is a collection of papers presented by him between the years 1930 and 1960, shows us in just how many fields he considers experimental psychology to be of immediate relevance. The range is wide, surprisingly wide. It stretches from the analysis of the conditioned reflex to a study of Shakespeare's sonnets and the design of cultures.

Skinner's emphasis upon the ultimate controllability of all forms of human behaviour may be distasteful to some, but it is difficult, though perhaps not impossible, to refute his argument that, as we are in any case controlled by forces external to ourselves, it is better that these forces be organized and beneficial than random and malignant, as they so often are at present. In reply to the really awkward question of what are the criteria by which control may be considered beneficial, Skinner seems to appeal either to the need for the survival of mankind, which he appears to consider unlikely without organized control, or to the need for a 'better' way of life, the achievement of which will presumably provide an operational definition of beneficial control. There are flaws in his argument, but it provides food for thought, especially in so far as it makes the psychologist ponder on his true function.

In more mundane vein, the current interest in teaching machines is well served by a full section containing four well argued papers which convincingly present the case for programmed learning. Prof. Skinner is a pioneer in this field, and these papers should be read by all who are interested in education.

In general, this is a stimulating book. The papers have been well selected and many are not readily available from original sources. The aim of the book is primarily to present a point of view. It is a point of view which we would do well to consider even if we cannot accept it.

R. S. CORTEEN

Morale in the Civil Service: A Study of the Desk Worker. By NIGEL WALKER. Edinburgh: at the University Press. 1961. Pp. viii + 302. 30*s*.

The title of this book is misleading. It leaves some very important morale problems virtually untouched. Among them is the one created by the Treasury's devotion to the 'general' class idea, which has the edge taken off its advantages by the fact that it makes many people qualify for promotion by doing work distasteful to them. One victim, 'job-rotated' into work with youngsters, said recently to the reviewer: 'I never asked to be put on this job. I took it because I thought I'd better. As a matter of fact, I hate kids.' Then there are the morale problems created for specialists—including many who are properly called 'desk workers'—by the unappreciative attitudes they find among their generalist desk worker colleagues. Evidence will soon be published which will suggest that fears about this and related matters make most university students of psychology put the Civil Service far down their list of desirable employers.

The content of the book is admirable. At long last a start has been made on research into management problems in the Civil Service. Moreover, it has been made by a very cultivated and perceptive senior civil servant who is a member of our Society. (Dr Walker was, until his appointment in 1961 to be Reader in Criminology in the University of Oxford, an Assistant Secretary.) Those of us who regard occupational psychology as basically concerned with the study of the satisfactoriness and the satisfaction of workers will be gratified by his contention that straightforward questions about these ('What makes for efficiency, collective or individual? What makes people enjoy their work?') are more to the point than windy argument about morale.

Using a questionnaire approach of a sensible, sophisticated kind, he has made comparisons between desk workers in two Ministries and two Companies. Among his variables were attitudes to discipline and promotion. Differences between the two Ministries and the two Companies were more striking than the differences between one Ministry and the other, or between one Company and the other. For instance, far more of the workers in private enterprise were satisfied; and far more of them would recommend others to follow their example.

Dr Walker gives a useful two-page summary of Dr N. F. Kristy's 1952 thesis, 'Criteria of occupational success among Post Office counter clerks', which he seems to think should have been published. May the reviewer, whose student Dr Kristy was at the time, express agreement? It is sad that no one has managed to persuade Dr Kristy to take the same view. However, the attempt continues.

ALEC RODGER

Struktur und Dynamik der Psyche. By WERNER FISCHEL. Bern and Stuttgart: Huber. 1962. Pp. 348. SFr./DM. 34.

This is a very difficult book to review. The author obviously has a reasonable and scientific outlook; he fully realizes the biological basis of psychology, and tries to integrate physiological and neurological studies with psychological ones. Yet any English or American reader looking through this book will get the very definite feeling of being in a very foreign country indeed, a kind of 'Alice in Wonderland', where people like Hull, Tolman, Burt, Bartlett, Osgood, Hebb, Spence, Spearman and many, many other household names have never been heard of. This makes a review almost impossible; this is the kind of thing Stout might have written, for all the relevance it has to what rightly or wrongly is considered modern psychology in this country. If this is the kind of text-book German students are brought up on, then there is little hope of any confluence between the European and the Anglo-American streams; they will have to continue flowing not so much along parallel lines, as on different planes.

H. J. EYSENCK

Angst und Schuld als Grundprobleme der Psychotherapie (*Anxiety and Guilt as Basic Problems of Psychotherapy*). By GION CONDRAU. Bern and Stuttgart: Huber. 1962. Pp. 189. SFr./DM. 26.

This Swiss author, starting from the thesis that anxiety is 'the illness of the 20th century' sets out to examine the fundamental importance of anxiety and guilt as modes of human being. His approach is a theorizing one, drawing sparsely on clinical detail. With extraordinary thoroughness he discusses the literature which has arisen around the two greatest theoreticians of human anxiety, Kierkegaard and Freud. The author's own position is an existentialist one. Psychotherapy aims not to eliminate guilt, but to integrate it into the individual's being. Though the discussion, which is on a wide basis, remains unproductive of a new line of thought, it makes very interesting reading; it throws much new light on the work of the psychotherapist and on the doctor–patient relationship.

STEPHEN KRAUSS

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(Inclusion in this list does not necessarily preclude later review.)

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Word and Object. By Willard, van Orman Quine. London and New York: Wiley. 1960. Pp. xvi + 294. 44*s.*

Conformity and Deviation. By Irwin A. Berg and B. M. Bass. New York: Harper. 1961. Pp. x + 449. 52*s.*

The Image. By Kenneth E. Boulding. Ann Arbor: University of Michigan Press. 1961. Pp. viii + 175. 10*s.* 6*d.*

The Nature of the Genius. By Andrew Gemant. Springfield, Ill.: Thomas. Oxford: Blackwell. 1961. Pp. x + 206. 52*s.*

Frustration. By Norman, R. F. Maier. Ann Arbor: University of Michigan Press. 1961. Pp. viii + 264. 12*s.* 6*d.*

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Television in the lives of our children. By W. Schramm, J. Lyle and E. B. Parker. Stanford: University Press. London: Oxford University Press. 1961. Pp. vii + 324. 32*s.* 6*d.*

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The Layman's Guide to Psychiatry. By James A. Brussel. New York: Barnes and Noble. 1961. Pp. xiv + 235. $1.50.

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The Literature of Possibility. A Study in Humanistic Existentialism. By Hazel E. Barnes. London: Tavistock. 1961. Pp. x + 402. 35*s.*

Sketch for a Theory of the Emotions. By Jean-Paul Sartre. London: Methuen. 1962. Pp. vi + 94. 12*s.* 6*d.*

Stress and Adaptation. An inaugural lecture delivered at the University of Leicester, 23 November, 1961. By S. G. McK. Lee. Leicester: Leicester University Press. 1962. Pp. 20. 3*s.*

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Note that World List abbreviations are used for journals.

The Oxford Dictionary is taken as the authority on spelling.

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OBITUARY NOTICE

REX KNIGHT, 1903–1963

His firsts and prizes at Sydney and Cambridge did not take Rex Knight far along the research path. The moment he joined the staff of the National Institute of Industrial Psychology, at the age of 22, he was launched on what turned out to be a remarkable career as a clarifier, an expositor and a facilitator of the work of others. The roles were of his own choosing, and his inclination towards them seems to have been evident from the outset; but they were eventually his undoing. The demands made on him became vastly greater than even the tough and resilient Rex could stand. His death in February was a disaster which must have been feared for some time by his wife, Margaret, who had worked with him from his earliest days at the Institute. Both appeared in the list of 'investigators' in the annual report for 1926. He was, in fact, assistant to the Director, C. S. Myers, and with him joint editor of the Institute's *Journal*: Margaret was listed on the title page as assistant to them both, and for a time she became joint editor with Myers when Rex left in 1928 to join Alec Mace at St Andrews.*

With Mace's full support, he moved on a year later, to become Anderson Lecturer in Psychology at Aberdeen, and to lay the foundations of the department in which he remained. From the start, he wanted psychology to be useful, and he believed it could be. He soon showed this, in a small book called *Intelligence and Intelligence Tests*. It is noteworthy that, thirty years after the publication of its first edition, it is still recommended by many heads of departments of psychology and education, despite the fact that it was written long before the present phase of the nature-nurture argument. It displays admirably his talent for digging down to the roots of a problem and formulating it in language the intelligent layman can understand.

At a later stage, he and Margaret produced jointly *A Modern Introduction to Psychology*, a vigorous, businesslike, informative primer which lived up to its authors' resolve to reject the advice once given by a professor to a thesis-writer, *Es muss etwas dunkel sein*. Its crisp lucidity and orderly comprehensiveness are of a kind usually achieved only by very hard work and self-criticism. He was sometimes too critical of his writing. He expressed considerable dissatisfaction with a lecture he gave eight years ago on children's needs and interests. It is safe to say that many who have read it, in *The Bearings of Recent Advances in Psychology on Educational Problems*, will have regarded it as a very good brief critical survey of current theories of motivation, by no means suited only for the teachers for whom it was primarily intended.

Rex's success as a writer and lecturer was no doubt attributable in part to the trouble he took to put himself in the shoes of his readers and listeners. 'Who are they?', he would ask before taking on a lecture, 'And what are their problems?'.

---

* Mace knew him better than any other of his friends, and the reader will be wise to regard this memoir as a supplement to the notice (by Alec Mace) which will appear in the Society's *Bulletin* (1963, 17, 3).

This did not seem to be born of ordinary 'sympathy', though he had plenty of that: it seemed to spring more from an acute awareness of the folly of arguing in woolly, undefined terms. He had not been a philosopher for nothing. Whether he was discussing psychology or the finer points of the theologies of Aquinas and Calvin, he liked everyone to be clear what they were talking about.

Probably the plain desire to get things straight, together with an urge to be useful, a concern with current affairs, and acceptability to people of all levels and types, made him the committee man *par excellence*. No one who served with him on committees is likely to question his pre-eminence, in such work, among present-day British psychologists. Have any of them ever done their homework more thoroughly or subsequently made their points more cogently? As a chairman he was superb. He was alert, incisive, firm, forthright, just. Nobody could complain that they had not had a chance to say what they wanted to say; but if it turned out to be irrelevant they would hear about it, gently. When he began: 'There are, it seems to me, four points on which decisions are required. First...', the old hands would glance at each other and at their watches: there would be fifteen, at the most twenty, minutes to go. Searchers for predictors of later performance in committee work may like to know that in his first paper, in the 1926 volume of the *Journal of the National Institute of Industrial Psychology*, there is a section which starts: 'There are three serious criticisms to which this experiment is open. First...'.

Such men attract others and inevitably become father-figures. So the load increases. It was not in Rex's nature to side-step the burdens he thus acquired. His advice was sought, not only by present and past students, but by colleagues too. Nor were the colleagues all academic. But in spite of it all, his company was exhilarating and gay. His many breakfasts with me in my Bloomsbury flat were a joy, even on the day on which, leaving the night train from Aberdeen, he had dropped his dispatch-case on the platform and drenched his DSIR committee papers with the whisky he had brought me. He had not dared to open the bag until he reached me, so I had to watch him pour the remains into my kitchen sink. One of the last times I saw him was on an evening on which he had managed to see *Beyond the Fringe* on a five-shilling standing-room ticket after a day of meetings. With a glass in one hand and an unaccustomed cigar in the other, he went through the show with hilarious mimicry; then, well after midnight, he suddenly embarked on a serious point-by-point comparison of Popes Pius XII and John XXIII. He made me dig out copies of *The Tablet*, from heaps of periodicals on my floor, so that he could illustrate his arguments by reference to recent letters which had seemed to him to be muddled.

Like A. W. Wolters, whose death was recorded here two years ago, Rex Knight became a powerful force in British psychology by his willingness to help others and his competence in doing so. Perhaps some day, when material becomes available for a factor analytic study of influential psychologists, we may find that the personal advancement of knowledge is only one important component of greatness, and that 'handling the advancers' is another.

ALEC RODGER

REX KNIGHT
1903—1963

British Journal of Psychology, Vol. 54, No. 3

ROBERT SESSIONS WOODWORTH
1869–1962

ROBERT SESSIONS WOODWORTH

1869–1962

Robert Sessions Woodworth died on 4 July 1962, at the age of ninety-three. His life included over sixty years of sustained, absorbed inquiry into psychological problems.

A major trend in his thinking was his persistent effort to conceptualize the subject-matter of psychology in such a way as to make it amenable, without oversimplification, to scientific research. One outcome of this endeavour was his interpretation of the relation between psychology and physiology. Early in his career, while working in both fields, he gradually became convinced that in their different accounts of an event—say, hearing a tone—the physiologist and the psychologist are describing the same identical process; that what the physiologist describes as occurring in the receptors and nervous system is the very process which the subject describes as hearing a tone and which the psychologist describes, perhaps as assignable to a point on a scale of loudness, perhaps as containing 'beats'. Similarly, in describing behaviour—say, a conditioned response—the two investigators give different descriptions of the same identical process. They use different techniques in their research and they report at different levels of description. Characteristically, but not invariably, the physiologist describes in greater detail, the psychologist in greater breadth. Both descriptions are necessary; neither can be substituted for the other; at each level of inquiry facts are disclosed which at the other are unobservable.

In this conception Woodworth seems to have achieved his basic orientation to his life-work. It enabled him to treat the subject-matter of psychology, whether observed introspectively or otherwise, as consisting of natural events in the natural world; and to see psychology as having, like physiology or any of the other natural sciences, its special function in their common enterprise. He first presented this view in a public lecture in 1908, and he subsequently stated it in several of his writings, including his last book.

Unless this basic orientation is taken into account—and it is often overlooked—Woodworth's treatment of some important problems may easily be misinterpreted. Woodworth is sometimes described as being 'on the fence'; as being so acutely aware of the complexities of a controversial issue as to be unwilling or unable to stand decisively for either side. He did not, in fact, join either side in the battle between Watsonian behaviourists and Titchenerian introspectionists. Yet he neither avoided nor evaded the issue. He met it by stating and defending a definite position of his own. From his point of view—essentially that indicated above—both sides made unnecessary and unwarranted exclusions; neither side admitted all the relevant empirical data obtainable. Characteristically, he stuck to the facts of the case as he saw and understood them—as he did on other occasions when his interpretation of a situation cut across the lines drawn by some controversy then in progress. On those occasions, too, some of his fellow-psychologists saw his treatment of the situation as unclear and indecisive.

In coming to terms more specifically with the subject-matter of psychology, Woodworth adopted first the *S–R*, then successive versions and various forms of the *S–O–R* formula. These he regarded as statements of psychology's essential task: that of accounting for the give and take between the organism and its physical and social environment. Psychological processes, he maintained, are characteristically directed toward an objective environment; they are essentially an organism's ways of dealing with such an environment. They include, in addition to motor behaviour, such activities as perceiving and learning. Perceiving the environment, for example, consists in dealing with it by transforming—or better, translating—sensory inputs from it into information about it, thus acquainting the organism with objective things, events and situations in an objective world. Learning, too, is a way of dealing with the environment. It consists largely in object-learning and place-learning, kinds of learning which are intimately involved in motor learning. Furthermore, these motor, perceptual and other ways of dealing with the environment require no motivation from sources outside themselves. Their activity is not dependent, directly or indirectly, on primary organic needs. They can and often do run on their own drive.

This is substantially the position Woodworth took in *Dynamic Psychology* (1918) in opposing McDougall's theory of the instincts. In 'Reenforcement of Perception' (1946) he presented perception as a motivated process, treating it as motivated intrinsically, as positively reinforced by its own successful outcome, and as not requiring reinforcement through any drive other than its own. In *Dynamics of Behavior* (1958) he took a further step. Here again he treated motor, perceptual and other ways of dealing with the environment as intrinsically motivated; but in addition he maintained that the tendency to deal with the environment is the primary and all-pervasive motivation of behaviour. Though the give and take between organism and environment is often directly or indirectly motivated by basic biological needs, the processes that constitute it do not require such motivation. They occur with it or without it, whereas they are themselves indispensable to the successful execution of behaviour which is driven directly or indirectly by primary needs.

No one believed more strongly than Woodworth that any theory, to be scientifically acceptable, must engage with the relevant facts. His two editions of *Experimental Psychology* (1938, 1954), the second with Schlosberg, indicate not only his respect for experimental findings, but also his comprehensive, detailed and critical knowledge of them. And going beyond experimental psychology in the strict sense of that term, he kept in touch, to an impressive degree, with developments in psychology generally. His extraordinary knowledge of psychological research was an integral part of his theoretical thinking, a fact clearly evident in both editions of *Contemporary Schools of Psychology* (1931, 1948). His own experimental investigations, some of them conducted with colleagues, included research on voluntary movement, imageless thought, 'racial' differences, transfer, and association. In addition, during the First World War, he prepared a *Personal Data Sheet* designed to screen out recruits whose precarious mental health might make them unfit for military service. Some of these studies were pioneering ventures into fields later developed by other investigators.

Many honours came Woodworth's way. Only one will be mentioned here. In 1956, 6 weeks before his eighty-seventh birthday, he received the first Gold Medal of the American Psychological Foundation. The citation referred to his 'distinguished and continuous service to scholarship'. It soon became evident that his distinguished service had not ceased. Two years later he published *Dynamics of Behavior*, the book in which he presented his soberly challenging interpretation of perception, learning, and motivation.

EDNA HEIDBREDER

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OBSERVATIONAL LEARNING IN MONKEYS AND APES

By K. R. L. HALL
Department of Psychology, University of Bristol

In natural groups of mammals, there is a variety of social influences which seem to inhibit or facilitate the expression of instinctive behaviour-patterns, and indeed which contribute importantly to the form the repertoire of behaviour characteristic of the species takes. Such influences include what goes under the terms 'social facilitation' and 'imitation', as well as other terms such as 'identification' which imply a special affectional relationship.

From an analysis of the evidence about these influences on monkeys and apes, it is clear that negative experimental findings have often been due to the limitations of the experimental procedures, and that some of the positive findings are difficult to interpret or assess for validity because of deficient method. By and large, there is convincing observational evidence, chiefly from informal developmental and from field studies, that young monkeys and apes acquire certain basic feeding and avoidance habits chiefly by applying their exploratory tendencies to places and objects indicated in the behaviour of their mothers or others of the group.

Although, in terms of learning operations, it is unlikely that much more than stimulus-enhancement or place discriminations is usually involved, a 'new' motor sequence almost never being in evidence, there is no doubt that the affectional situation of the participants is crucial as to whether a 'demonstrator' is attended to, closely approached, and 'imitated', or not. The nature of the affectional variables and their influence upon learning require extensive experimental study and analysis, and a comparison with related forms of early learning, such as imprinting, can then be made.

INTRODUCTION

In the natural social groupings of animals, at least at the vertebrate level, there is a variety of 'conformity' processes going on, some of which seem to involve arousal of innate propensities or tendencies through the apparent occurrence of innately recognized social releasers, while some, perhaps at the 'highest' level—that of the anthropoid apes—seem to be ascribable to individual learning. All would appear to have in common the function of co-ordinating and 'conventionalizing' behaviour so that a social 'norm' or central tendency is achieved. Before considering in detail the evidence for such conventionalizing in monkeys and apes, it seems logical to suppose that we are dealing here with a fundamental evolutionary tendency, the result of which is to produce individuals within a population, within a total species-distribution area, in whom can be discerned, in the very nature of things, so-called species-characteristic behaviour-patterns. But, further, within the actual life experience of individuals within a group, there is a pattern of habits relating to feeding, sheltering, etc., determined largely by the ecological circumstances of the group, in which the details, at any rate of sign or external-stimulus 'meaning', are filled in by the learning of the individual through observing that which other, usually 'senior' in the literal sense of 'older', individuals of the group do in the course of their daily routine. Aberrant, or eccentric, individuals, more venturesome (i.e. exploratory) than the rest of the group, may conceivably be instrumental in starting off a new and advantageous feeding habit or habit of avoidance, and this may sometimes percolate through the rest of the group until, eventually, the 'new' habit is a part of the group norm.

Now, the generality of such normalizing tendencies in human societies has been heavily emphasized by Darwin (1874), Tarde (1904), McDougall (1908), and others. But the comparative assessment of the importance of such tendencies in non-human animals has been somewhat neglected. Axiomatically, it seems likely that the origins, in individual mammalian life experience, of the conventionalizing tendency come early in ontogeny as the infant animal learns to achieve a certain independence of its mother, having of necessity conformed to the mother's control during the period of weaning, and having, inevitably in the natural group situation, observed almost exclusively the behaviour of those closest to the mother. Then, as the young animal matures, its spontaneous and characteristic playfulness leads it to learn certain social habits in the rough-and-tumble of the play subgroup, but its physical survival habits, such as those connected with feeding and drinking and avoidance of the noxious, are probably early established in the daily process of observing the mother and her immediate associates.

All this sounds obvious, but so suspiciously obvious that one finds the real nature of the conforming process in the higher animals has by no means been elucidated experimentally or in the course of naturalistic studies of wild groups. Only in the most general terms have the complex processes involved been described, and, within the broad comparative framework, it has been usually assumed that *Homo sapiens*, then the anthropoid apes (with first and foremost the chimpanzee), then the monkeys, and so on, with also-rans in the carnivores, such as cats, benefit, in that order, by the individual acquisition of basic conformist attitudes which thus are a product of acquired 'tradition' rather than of a genetic endowment.

The evidence, however, is chiefly of the informal and even anecdotal kind of reliability and validity, and few attempts, so far, have been made to link up 'imitative learning' and its origins with, say, imprinting in birds and any corresponding tendencies in young mammals, although Scott (1958), amongst a few, has been looking for the origins of social attitudes, at least in dogs, in a 'critical physiological period' of ontogeny during which the conventions of the group are established. The isolation method, however, has serious disadvantages for our purpose, because, in its usual form, it consists of depriving the young animal of exactly the characteristic social environment of mother and litter-mates for which its propensities have been evolved to fit it. Hence, the failure to find certain 'characteristic' behaviours emerging in an isolated young animal certainly does not mean that the germs of these behaviours are not in its repertoire. On the contrary, as is known nowadays from many mammalian studies, the lack of suitable *stimulation* from the mother may result in the tendency inherent in the young animal never fully emerging.

One is, of course, immediately in danger of circular argument about the complex interactions of the genetic endowment with the variety of environmental circumstances necessary for eliciting its full potential, and it is probably best simply to avoid the issue by postulating that the genetic endowment *normally* fits into a more or less constant sequence of mother-contact, mother-feeding, etc., stimulating situations, and that this sequence radiates out into the usual situation of playing with and learning about a lot of other members of the group of varying degrees of seniority and status.

It will not here be possible to attempt to relate the evidence on the monkeys and apes, such as it is, to the human species, on the one hand, or to the conventions set up by the non-primate animals, but it is hoped that the picture achieved at the end will lead to at least an elucidating critical programme and a programme for experimental research. Before beginning to review the evidence, it is necessary to deal with a few of the many terms that have been used to describe conventionalizing tendencies.

TERMINOLOGY

Hediger (1950) refers to the 'contagious' effect of the sudden occurrence of an eccentric feeding behaviour in mammals. For example, one member of a herd of red deer starts to strip bark, and the behaviour spreads rapidly to other members of the herd. He quotes evidence which indicates that 'safe' habits are learned, in some herd animals, and passed on from adults to young, but no attempt has yet been made to analyse experimentally the process of transmission although a reasonable hypothesis would be that it derives from the following tendency characteristic of the young of ungulate species.

The term 'mimesis', on the other hand, apparently refers to an 'instinctive' sequel of more or less identical behaviour being aroused by observing this behaviour's occurrence in another of the species, but without the observer-animal necessarily perceiving that which *releases* the sequence in the demonstrator. Verplanck's (1957) definition of 'behaviour/mimetic' is as follows: 'An animal is placed in the environment of another animal of the same species: if the second animal then makes some species-specific response, and thereafter the first animal makes the same response under conditions where it can be demonstrated that the response of the second animal is the stimulus for that of the first and that no opportunity for discrimination and differentiation training has been given to either animal, then the behaviour is termed "mimetic"' (p. 4). It will be noted that Verplanck's apparently scrupulous definition does not say that the behaviour of the first animal is more or less exactly what the second animal then does, but presumably this is intended, for otherwise we would be dealing here, in many instances, with a form of social conditioning. Mimesis seems to be synonymous with what the older writers called 'instinctive imitation'. Thus, Washburn (1908) diagnoses the latter's occurrence '...when the sight or sound of one animal's performing a certain act operates as a direct stimulus, apparently through an inborn nervous connexion, to the performance of a similar act by another animal' (p. 238). As an example, she cites Lloyd Morgan's description of one of a group of chicks which learns by casual experience to drink from a tin of water. Other chicks will then run up and peck at the water and will themselves drink. Although, therefore, the *action* is instinctive, in the sense that it is a part of the natural repertoire of the chicks independently of their observing its occurrence in other chicks, individuals do learn *where* to peck, and *what* to peck at, sometimes by means of the gregarious tendency to do what another chick, or perhaps the adult hen, has been doing. This, we shall see, seems to be more or less exactly what is meant by 'imitative learning' in some accounts of the behaviour of monkeys and apes, for it involves nothing new or even modified on the *motor* side, but it does involve a social *focusing* on the stimulus side. Koffka (1924) likewise summarizes his views on imitation by distinguishing between cases where configurations already belonging to an individual's equipment are made to function by the performance of an act of the same kind by another individual, and those where a *new* configuration is aroused as a result of the observer perceiving another action in a certain manner. The former category may itself be subdivided into instinctive and acquired.

Hence, one would suppose that this process also is more or less the same as what goes under the terms 'social facilitation', defined by Crawford (1939) as any increment of individual activity which results from the presence of another individual. But this is a very broad inclusive statement, and would cover the obvious example of the puppy that has eaten all it apparently requires returning with avidity to the food-trough as soon as another puppy begins feeding there. Is no learning involved here? One would be rash to suppose this is due to a straightforward arousal of an instinctive tendency, for the social facilitation is presumably itself determined by the early litter experience of the animal.

And what of the process known as 'empathy'? Mowrer (1960) defines empathy as an 'imitation of affect' (p. 115), and it is thus distinguishable, so he implies, from the sort of imitation of, say, an instrumental conditioned response, the use of a tool, or a discrimination learning task. Experiments supposed to exemplify this kind of emotional limitation are, for example, Church's (1959) study which demonstrated that rats experience fear, as indicated by behaviour inhibition, in the presence of other fearful rats, and that of Miller, Murphy & Mirsky (1959) who taught monkeys to operate a lever when afraid, and who showed that such behaviour can be activated by the mere sight of another monkey in a state of fear. Thompson (1958), however, attempts to raise empathy to a rather 'higher' form of behaviour in his interpretation of one of Köhler's (1925) reports of his chimpanzees, where one chimpanzee was watching another reaching for fruit hanging at a precarious distance away, and was able 'to identify itself with its fellow to such an extent that it made spontaneous reaching movements in empathy with the other animal' (p. 295). The empathizing animal must not only be keenly aware of the other individual, but must be able to act as if he were in the place of the second animal. 'Thus empathy demands from the individual the ability to dissociate himself from his immediate concrete environment and to react to an absent or hypothetical one' (p. 295). One may question whether it is necessary to infer, on the behavioural evidence, the 'awareness' or the 'dissociation' that Thompson refers to in this instance, for no thorough attempt at a constructive experimental elaboration of such performances has yet been made.

Yet another 'emotional' aspect of the conventionalizing tendency goes under the term 'identification', for it is assumed by Mowrer (1960) and others that the individual, usually a young one, will learn a great deal by watching, and perhaps copying, the actions of a person, or animal, with whom it has identified itself, i.e. sees itself as having the same role. Possibly this also emerges from the 'filial' or 'following' tendency in some young animals, but goes for much more than mere 'following' when it involves the acquisition of feeding habits, of aversions, and so on.

In a very loose sort of way, these various conceptions indicate that something socially (conventionally) important by way of early learning goes on in intimate relationship with positive emotional attitudes or 'affectional' responsiveness, such as concerns alleviation of anxiety—perhaps as in contact-comfort seeking, and in just drawing near to a dominant but protective animal such as a mother or another 'sympathetic' adult. The real distinctions between the variously named processes of instinctive imitation—mimesis, social facilitation, empathy and identification—are not clear, and one would suppose that the processes involved in imprinting and any mammalian parallels to this must be considered in relation to them. The quite difficult effort must undoubtedly be made to view the various phenomena in which a young animal gets itself usually spatially close to another, or is visually, though at a little distance, 'interested' in another animal, not as though they were self-explanatory, but as though they merited the most thorough experimental and observational attention.

We might finally, in this section, concern ourselves briefly with the kind of imitation in which the animal, or person, does apparently do something after observing, which does not fall within its habitual repertoire. Köhler (1925) and Koffka (1924) would both seem to deem this as a special case of insight learning, for the new configuration is achieved, not simply by a reorganization of parts of the problem, as in the tool-using or box-stacking situation, but by perceiving the reorganization in the performance of an experienced demonstrator. Similarly, Washburn (1908) defines inferential, or reflective, imitation as occurring '...where an animal, watching another one go through an action and observing the consequences, is led to perform a similar act from a desire to bring about the same result' which, as she says, does not really differ from '...any other case of learning by ideas' (p. 239).

In effect, we find, as is clear from the review in Miller & Dollard (1941), that imitation is so broad a term that it can be made to cover almost any kind of performance where the behaviour of an observer has apparently been influenced by the behaviour of a demonstrator. The defined result is usually a facilitation or improvement of some kind on the part of the observer, but it may be a decrement or inhibition, as in the case of fear-responses. It is clear, however, that, although the *operations* whereby the observer learns, where it does learn, thus to modify its own behaviour can be variously classed with conditioning theory of some sort, as by Humphrey (1921), Pavlov (ed. Koshtoyants, 1955), or Mowrer (1960), with recourse to latent learning and secondary reinforcement in the latter case, or with field theories of the gestalt kind, as in Köhler (1925) and Koffka (1924), the essence of the importance of the assimilative or conventional behaviour within the group is simply overlooked in such formulations. The tendencies apparent under the various terms are far from easy of explanation, for the reason that they are so simple-looking, *and so familiar*, that they are taken more or less for granted. *Why* does the infant animal watch the mother feeding, or watch her flinch away from a dangerous object? Because it 'identifies' itself with her? What, in terms of learning, does this process of identification, or empathy, or mimesis, or social facilitation, really involve? Hebb (1960) asserts that the mental processes of self-perception are the same processes, in large part, that constitute the perception of another person, and '...the sight of a hand making some movement is inevitably associated closely with the corresponding somesthetic sensations, when the hand is one's own, so that the sight of another making the same movement must arouse the same central process that causes one's own movement—and tend to produce imitation' (p. 742).

Perhaps one of the simplest ways of arriving at some sort of explanation of what goes on in imitation, or what we can very generally call 'observational learning' in an attempt to avoid the conundra associated with 'imitation', will arise from studying the ways in which monkeys and apes try to deal with what goes on when they perceive themselves performing in a mirror.

THE EVIDENCE ON MONKEYS AND APES

Anecdotal

The general supposition amongst animal keepers and others who have informally or casually studied the monkeys and apes has always been that they readily imitate one another and that they also learn to copy the actions of human beings. There are many examples of which a few only need be cited, for they chiefly bear upon the initially important questions of method which we shall see below account for much of the discrepancy in the more formal experimental laboratory findings.

Romanes (1882) quotes his sister's description of how the Cebus monkey she looked after learnt to open a stiff-lidded trunk, undoing it with a key, mainly by *copying* the actions of the sister's mother. Similarly, Witmer (1910) supposed that his *Macaca cynomolgus* monkey learnt to open a greenhouse door, by hanging down by her hindlegs and thus reaching the latch, without preceding trial-and-error, through '...making an intelligent attempt to imitate the persons whom she had seen going in and out of this door' (p. 225). He goes on to suppose that this is a conscious, intelligent process. This interpretation is heavily criticized by Watson (1911) who describes this paper as a return to the worst type of anecdotalism with no attempt to control the experimental conditions. Shepherd's (1915) observations of a chimpanzee fall into the same category: 'As a test of imitation, I took out my watch and pressed on the stem, slowly, and opened the watch three times while Peter (the chimpanzee) watched my actions with attention and apparently with interest. Then I reached it to him; he held it and pressed on the stem correctly several times, as if to open it. However, he did not press hard enough, and the watch did not open. He thereupon attempted to open it with his finger nails. The keeper stated to me that the ape had not received any training on that act' (p. 393). Furness (1916) recorded observations on intelligent imitation in two young orang-utans and two young chimpanzees, and stated that simple actions, such as digging with a spade or trowel, scrubbing, sweeping, screwing in a screw, were learned by the female chimpanzee 'entirely by imitation' (p. 289).

Such observations lack controls, such as adequate knowledge of previous experience of the animals studied, and lend themselves readily to the preconceptions of the particular investigator as to the animal's intelligence. Nevertheless, we shall find that the fact of learning such performances as these, partly by observing humans doing them, seems clearly established, although even some of the 'rigorous' experiments are surprisingly naïve in their design and in their neglect of rather obvious factors such as the amount of improvement that could be made by the animal on such a task, without opportunity for *seeing* it performed. There are two things of some importance about these sorts of observations. One is that they are carried out on animals with whom the 'experimenter' is on very friendly terms. The other is an interesting point made by Furness (1916) who says that the use of spoons, forks, needles, etc., which seems to come so readily to at least the anthropoid apes that live in close contact with human beings, may represent '...but slight modifications of instinctive actions of use in the pursuit of food, or to satisfy a natural curiosity. A twig or a stick may be poked into a hole to pry out a grub...a drink of water out of the hollow of a leaf is like drinking from a cup....Therefore, I do not think that such actions demonstrate any marked degree of intelligence' (p. 289). This surprisingly modern view is amply confirmed in recent work (as, for example, Morris-Goodall's, 1962, report on the feeding behaviour of wild chimpanzees). It is, perhaps, even more surprising to find that Mills (1899) is able to defend his naturalistic approach to animal intelligence in general, and to imitation in particular, in a way that would do credit to a present-day ethologist. He points out that the anecdotes of animal trainers and others, '...though their reliability must be checked' (p. 262) and though 'caution in conclusion is necessary', should not be discounted simply on the results obtained in highly restrictive and unnatural experimental situations such as those provided by Thorndike (1898). An animal cannot, he says, be expected to behave naturally or in terms of its real capacities unless it is viewed in a natural kind of environment. Further, one great fallacy in Thorndike's experiments is that '...he overlooks the many possible and actual inhibitions to response to a stimulus' (p. 267). And: 'Even to witness a performance of trick animals is enough to enable one to see how at one time a tendency to imitate assists and at another mars the performance' (p. 268).

Experimental

The dividing line between casual observation and true experiment is obviously an artificial one, as indeed sometimes is the division between naturalistic observations of a more comprehensive kind, such as those of Yerkes, and controlled experiment. Nevertheless, it is convenient to consider the 'experiments' in laboratory-type situations as distinct from the free situations in which the above early results were obtained. The chief methods used in studying imitation in mammals are set out by Adler (1955) as follows: (1) the single-cage method, where observer (*O*) and demonstrator (*D*) are together in the problem-situation; (2) the observation-cage method (Thorndike, 1901), where *O* watches *D* do the problem, and is then transferred to the same problem-cage after *D* has been removed; (3) the duplicate-cage method (Warden & Jackson, 1935), where *O* is in a cage, watching *D*'s performance, and is thereafter able to work on the same problem, from its own cage, without the delay and disturbance necessitated by method (2), usually through the watching-screen being taken away so that *O* then has immediate access to the problem.

But variation in method is, as we shall see, not the only reason for discrepant experimental results. Other reasons are suggested by the anecdotalists, and some will be found to bear simply upon the sheer artificiality of the imitation situation which so often takes no account of the probably critical social, emotional, and general motivational factors involved.

The experimental studies can be roughly divided into those which claim to have demonstrated imitative learning in monkeys and apes, and those which are unable to demonstrate its occurrence. Typical of the latter are those of Thorndike (1901) and Watson (1914). Thorndike used the same instrumentation tasks as in his 1898 study of cats, dogs, and chicks. He carried out only *five* experiments relating to imitation with *one* Cebus monkey only as an *O*. As *D*s, there was himself and also two Cebus monkeys. At the time of the experiments, *O* was 'on terms of war' with one of the two *D* monkeys. Using method (2), *O* had the chance to watch *D* carry out, correctly, a manipulatory problem, and then *O* was moved into *D*'s cage, *D* having previously been removed. Hence, there was obviously delay intervening between observing and trying to perform what was observed, together with the very strong likelihood of arousing motivation inhibitory of any kind of experimental performance in the process of being moved from one cage to another. Thorndike's negative conclusion that on none of the experiments did *O* gain any benefit from observing *D* must be taken as more or less valueless: 'Nothing in my experience with these animals then favours the hypothesis that they have any general ability to learn to do things from seeing others do them. The question is still an open one, however...' (p. 42). He noted also that *O* showed little *curiosity* about *D*'s performance, and, in fact, this lack of interest in each other's activities struck him very forcibly in all three animals, while he goes on to note that probably imitation would most readily be observed as between parents and offspring. The limitations of these experiments are so obvious, when seen in context, as to need no further emphasis. They have all the general flaws of the anecdotal observation—such as too few cases, lack of knowledge of previous life experience, and so on—without the merit of at least observing the animal in a 'social' setting where a 'social' form of learning, such as imitation, would be likely to occur.

Watson's (1914) experiments are of the same order. He used four monkeys, selected out of nine, because of their gentleness or alertness. One of the four was a baboon which he said was so stupid that its experimental performances were not worth considering. One was a Cebus, and the other two were Rhesus. Manipulative tasks were used, and Watson himself served as *D*, except for the two Rhesus who were set to imitate each other. In no instance did an *O* show any gain from watching performances on the tasks, including correct solutions, but the whole situation was so unnatural as to preclude the likelihood of any imitation occurring. No consideration was given to the possibility that the performance of *O* might be *inhibited* by the setting—which included the visible presence of the experimenter even when *O* was supposed to watch a *D* monkey.

To offset these two negative studies there have been a number of experiments, beginning with those of Hobhouse (1901), which demonstrate that some form of increment of performance is apparent in the *O* monkey or ape as a result of observing the practised performance of a *D*. It is by no means always clear, however, exactly what it is that elicits this improvement, and whether it is really an improvement due to observing *that particular performance or set of performances* rather than to some factor relatively independent of the actual task, such as a sheer increase in 'interest' in the task because of the presence of another monkey doing *something*. The findings are therefore of very varying degrees of importance.

Kinnaman (1902, I and II) succeeded in getting two Rhesus monkeys to imitate one another's success on a box-opening problem, but not to benefit by observing the experimenter doing this task over and over again. Thus, the female Rhesus was said to have 'copied' the act of the male in pulling out a plug in a more or less precise replica of the way he took hold of it and the direction in which he pulled it. 'He went immediately to the box, she following some four feet away. Knowing the trick perfectly he seized the end of the plug with his teeth and removed it. I set the box again; this time the female rushed to it, seized the plug by the end as the male did, and procured the food. This she repeated immediately eight times in exactly the same way' (p. 121, I). Other than that, however, he was only able to discern 'instinctive imitation', as where the female 'took a peep in exactly the same way' when the male looked under the bottom of one of the trees in the room.

Haggerty (1909) produced positive evidence of imitative learning in a study using eleven monkeys of different species. These animals were grouped in small cages, '...the aim being to secure for each congenial cage-mates' (p. 347). Eight simple mechanical problems were set for the animals, with the following experimental procedure: (1) each monkey was given five preliminary trials of 15 min., on successive days, on a particular problem, at the end of which most of the animals had either succeeded or become indifferent; (2) 'imitation' tests were then begun using those monkeys 'that had failed to learn of their own accord' (p. 353), the trained *D* performing in the presence of *O*. Of the eleven 'failed' monkeys, nine were successful on the tasks after demonstration performances. The obvious difficulty about these results is that they may merely be eliciting latent learning or social facilitation in the new 'social' situation of being paired in the performance with another monkey, instead of performing alone. He does, however, pay heed to certain social factors that might be relevant in imitation experiments, such as the degree of congeniality or animosity between *D* and *O*, but is unable to show convincingly how these factors might operate in facilitating or inhibiting performance. He goes on usefully to grade at least four different levels of imitative behaviour, beginning with simple arrest of attention by looking at another monkey and ending with exact repetition in detail by *O* of *D*'s performance. These results cannot be considered at all conclusive because, as Zuckerman (1932) points out, the success of the *O*'s may be due to sheer perseverance or simply to the tendency of *O* to *follow* another monkey when it goes for the 'key' to the problem, i.e. 'stimulus enhancement' (Spence, 1937).

Kempf's (1916) experiments, using six Rhesus monkeys, are also inconclusive, for, using method (1), it cannot be clearly established that anything more than the *place* relevant to solution was what the *O* learned, by watching the successful *D*, to discriminate. Indeed, Kempf says as much in concluding: 'Imitation seems possible only when similar movements under similar circumstances have already been previously acquired by the organism. Imitation seems to permit of a very limited margin for new modifications of the old repertory of movements' (p. 265). Kohts (1928) likewise (quoted by Zuckerman, 1932, p. 165) states that, for example, demonstrative unlocking of a device does not teach the monkey anything, but only *stimulates* him in the performance of his task and shows him a definite spot where some kind of work is to be fulfilled.

Only with the studies of Warden & Jackson (1935) do we get a clear analysis of at least some of the essential controls and criteria to be used in imitation experiments. This is the first thorough use of method (3), duplicate cage, on monkeys, the problems used being of the usual simple mechanical kind, such as pulling a chain which hung down within easy reach of the front of a panel, thus raising a door and exposing a raisin in a hole. *O* was allowed only 60 sec. to perform the task, after *D*'s trial, *D* having been previously trained to correct performance. The *same animal* served as *D* throughout for all of 15 *O*'s. This was in order to try to keep the social factor more or less constant, for: 'Although somewhat nervous in temperament, his [*D*'s, a male about 43 months old] performances were consistently definite and speedy. He was especially suitable for our purposes since he did not attempt to fight back when the other animals exhibited pugnacity from time to time' (p. 110). However, they note that it might have been better to have trained a female to serve as *D* for the females for several of the older females tended to show more interest in this male than in the problem: 'This factor probably had the effect of reducing the score of imitative behaviour in such cases below the potential level' (p. 110). Prior to the imitation tests, each animal had been caged alone 'so as to prevent the formation of social attachments that might prove troublesome' (p. 111).

For the first time, explicitly in the method, an attempt is made in this study, in at least a preliminary way, to control the social variables which are thus recognized as being significant influences upon imitation. Their results show successes by *O*'s, within 60 sec. (= immediate imitation), in 46·3 % of the 354 tests. This figure was raised to 72·4 % when the records of six *O*'s were eliminated because of serious sexual distractions.

In order to distinguish 'intelligent imitation' from other social effects on behaviour, five criteria must be met: (1) the task must be novel and sufficiently complex; (2) the response must appear immediately after *O* has observed the *D*; (3) practice must be excluded by the experimental conditions; (4) the act of *O* must be substantially identical with that of *D*; (5) a sufficient number of instances must occur, under varied conditions, to eliminate the chance factor. No experiments before this could be said to have fulfilled these conditions, but it still seems that results such as those of this study might be explained on the basis of *some* kind of previous experience, on other sorts of problem, which, combining with the extra incentive of *D*'s *presence* and movements (and not necessarily his actual correct performance), bring out very readily a correct solution. As in other studies, the essence of the problem is simply to locate the meaningful cue, such as chain or latch, and the experimental fiddling of the active animal would do the rest. The line between 'social facilitation' and 'intelligent imitation' is certainly not entirely clear, for the persistent exploratory–manipulatory behaviour of these animals must build up an extensive repertoire, through stimulus and response generalization, some aspect of which can readily manifest itself in the so-called 'novel' situation. However, as Zuckerman (1932), quoting from Adams, points out, the mere location of the key area or object through the action of a *D* leads to an altered attitude on the part of *O* through '...amplification of perception, accrual of meaning or enhancement of context' (p. 170). This, in itself, is due to the continuous social conditioning of natural monkeys or apes, and is, psychologically and ethologically, just as important as any evidence of imitation of a more deliberate and 'conscious' kind.

Another study by Warden, Fjeld & Koch (1940) used the same method and procedures on six monkeys, all of which, as was the case with the 1935 animals, had taken part in previous instrumentation and problem-solution experiments, though none of the present experiments resembled in *detail* those on which they were experienced. Instant imitation (within 10 sec.) occurred on 40 % of tests, and immediate imitation (within 60 sec.) on 76·4 %—a very similar result to that of 1935. It is thus assumed that, in addition to the 'social facilitation' evident on all the tests, there is an extensive 'copying' of *D*'s performance. As with the other study, however, it seems that we have the following factors at work: social facilitation, previous practice, and local enhancement.

So far, imitation has been studied in terms of performances involving some kind of manipulation, the schemes for which are undoubtedly within the genetic and habit repertoire of the animals. In addition, simple discrimination-learning tasks have also been used in the *O* and *D* setting on monkeys and chimpanzees. Crawford & Spence (1939) used a new method in which an *O* chimpanzee, previously trained in discrimination learning, is given a series of chances to observe another (*D*) chimpanzee's differential responses to a pair of stimuli, *D*'s choices being irregularly rewarded with food. The duplicate-cage method was used, *O*, on a series of trials, being able to watch *D* (who could not watch *O*) press down on a plate of different shape or colour, and being then given the opportunity of itself responding to one of two stimulus-plates. Results on nine animals are said to be inconclusive and variable, and to provide little evidence of imitation over and above effects attributable to the social facilitation (through *D*'s *activity*, as such) '...adding, in some way, excitatory strength to the chosen stimulus' (p. 144). Two of their incidental comments are of some interest. The first is that the 'friendly' relation between the *D* and *O* of a pair led to immediate improvement on imitation trials by *O*, in contrast with the same *O* observing another *D* animal. The second is that movements of the experimenter were suspected of acting as secondary cues for one of the *O*'s imitation trials, and, when this was controlled, *O*'s performance dropped from 75 % correct to chance.

In contrast with this inconclusive result on chimpanzees, Darby & Riopelle (1959), using similar techniques and problems on four experimentally naïve Rhesus monkeys, found that *O*'s learned increasing amounts from their single observation trial on each problem, and, further, where the *D*-trial was *not* rewarded, *O* was significantly more successful than when following rewarded *D*-trials. Thus they conclude: '...Rhesus monkeys can acquire information about the solution of an object-quality discrimination problem simply by watching another monkey execute a single trial on the problem' (p. 96). As they point out, Spence's (1937) notion of stimulus-enhancement does not fit the results, for *O*'s performance is better if *D* makes a mistake than it would be if *D* made a correct guess, and they suggest that *O* is distracted by seeing *D* getting a food reward. The design of these experiments is, of course, such as simply to test out the degree of *following* by *O* of differential responses made by *D*. Though technically ingenious, they contribute little that is meaningful on the nature and extent of the imitative processes. This restriction also applies to Riopelle's (1960) study of four experimentally naïve female Rhesus monkeys using the same apparatus as in the 1959 study and selecting from 100 pairs of identical objects. Each monkey was used as both *O* and *D*, and the degree of following of *D*'s position choice by *O* improved to above chance level (63 % correct), whereas the *D* animals, who were learning 'by participation' showed a rather better rate of 'correct' (food-rewarded) position choices.

Little, so far, has been done to elucidate how imitation may affect the avoidance-response learning of monkeys and apes, and this area of study seems highly likely to elicit important results, for ability to learn to avoid a noxious object through observing the avoidance by another animal of the group would seem of great survival value. The 'contagion' or 'facilitation' consequent upon alarm-barks or other species indications of fear in the animals is well documented, though not well analysed. Haslerud (1938) says of chimpanzees: 'Species tradition or group experience must also be important in increasing avoidance reaction to inanimate objects (as it does to moving objects—the subject of that particular experiment). Learning through species tradition occurs particularly well in chimpanzees because there is so much social facilitation and inhibition. Let one chimpanzee see any object that scares it and elicits the fear cry, in a moment the whole colony is in flight and takes up the cry...species tradition may increase and reinforce a persistent caution toward objects' (p. 526).

Only one adequate experiment to test out a very limited hypothesis about imitation and avoidance-learning has yet been reported on monkeys. Presley & Riopelle (1959) trained *D*'s to avoid electric shock by leaping over a barrier within the first 4 sec. following appearance of a red light, until they made twenty-eight avoidance responses in a block of thirty trials. The *O* watched the whole performance from an adjacent compartment and was then transferred to the shock compartment. Results showed that the slowest *O* learned the avoidance response in as few trials as did the fastest *D*: 'these data thus show that Rhesus monkeys learn an avoidance response more rapidly after having observed another learn it than they would have without this observational experience' (p. 253).

Experiments by Miller *et al.* (1959), using what they call 'inter-animal' conditioning techniques, produced the same sort of result, one monkey serving as the CS for avoidance behaviour on the part of a second monkey. Further, pictures of 'fearful' posters and expressions tended also to elicit more avoidance responses than non-fearful ones. Hence it is concluded that the fear affect can be communicated from monkey to monkey through observation of 'fearful' monkeys or pictures of them, but this seems, in a complicated way, to tell us little that was not already evident from natural observation.

We have seen that the social factors which might very materially influence the form and extent of imitation have scarcely been investigated. Remarks about their possible significance have occurred in a few studies, and in one, at any rate, the attempt was made to hold the social factor constant. The conventionalizing tendency is so obviously a product of the social group climate that it is surprising to find how little has been done here. One of the earliest experiments is that of Aronowitsch & Chotin (1929, quoted by Spence, 1937, p. 822): 'A single dominant male [monkey] and a group of 3 others were given opposed training. The former was trained to open a food box when light *A* was present and not when light *B* was present, while the group of 3 were trained in the reverse manner. The subjects were then all placed in the same cage and the conflict between their learned behaviour and the opposed social example observed. It was found that the average amount of imitation was 25 %, and, furthermore, that the individual animal imitated the group more than he influenced them.'

There is nothing very surprising in this result but it does suggest that independent behavioural evidence of the relative dominance of an *O* and *D* pair in an imitation experiment should be obtained at least *after* the experimental trials were concluded.

Nissen & Crawford (1936), and Crawford (1937), made very valuable experimental attempts to work out, respectively, how habits of food sharing and co-operative problem solving could be trained in young chimpanzees. In neither of these studies is imitation specifically studied. In the former study, it was found that transfer of food and food-tokens from one cage to another of a pair of chimps occurred according to any one of three modes of behaviour: independent acquisition, begging, and unsolicited passing. The results provide evidence for mutuality of behaviour, and hence for the kind of conformity tendency that seems to be the crux of the imitative process. In Crawford's study, there is much more of direct relevance to our topic, for the essence of the co-operative work is the learning both to pull and to pull synchronously with the other chimp, or with the experimenter, and this is achieved largely through 'watching' the other animal: 'This "watching" behaviour...has been considered the distinguishing characteristic of the second stage in learning to co-operate, in which the animals first showed a response to each other' (p. 25). In neither of these studies is anything 'new', either of movement or of discrimination, achieved through observational learning, but, nevertheless, a new achievement over and above the capacity of each of the two animals individually results from the social interactions of the two.

Recent social-experimental studies on Rhesus monkeys by Mason (1960, 1961) relating to effects of social restriction on the general behaviour-repertoire, including sexual responsiveness, strongly emphasize the degree to which adolescent or adult monkeys learn conformity habits by imitation of other monkeys. Indeed, these few social experimental studies all indicate clearly that the true problems of 'imitation' in monkeys and apes have been largely neglected by experimenters who have been more concerned with a kind of 'school learning' idea of benefiting from 'demonstrations' on 'problems'. What is of fundamental importance is to know how simple, basic habits of avoidance, of mutual tolerance, of feeding, and so on, are formed in the context of the group through *observing*, and not necessarily, at the time, *copying*, the behaviour of members of that group.

Naturalistic observations

The experimental findings examined have been somewhat barren, not so much because of the attempts at standardization and control of conditions as because the most interesting factors, social and affectional, were not usually the concern of the investigators. Free investigations or observations, on the other hand, tend to produce highly suggestive results which are, as they stand, simply hypotheses which may be studied in detail so that the complex of factors probably underlying the naturalistic data may more clearly emerge.

The data under this heading includes that from full-scale field studies in the wild, studies of captive animals in a semi-natural setting, and observational studies of animals living in laboratory or cage conditions or in human homes. It is here we find some information—completely lacking, so far, in the experimental work—about how the young animals may learn to imitate others or adults, while some important inferences can also be drawn about the probable function of imitation in creating the 'traditions' of various free-living groups of monkeys and apes.

Developmental studies

Most of the observations relating to imitation are incidental to the general purpose of assembling all relevant life-history data on the young animals. One of the earliest of such studies is that of Lashley & Watson (1913) whose only observations directly bearing on our problem are as follows: 'Dolly's [the mother Rhesus] movements sometimes served to draw the baby's attention to some object, but his own reactions to it never seemed to imitate hers. At times also, his responses appeared to be determined by those of adults, as when Dolly's threatening attitude after some acts of the observer induced fright in him although he had not shown fear of the act itself. His response in such cases was, however, quite different from that of the adult and in no sense imitative' (p. 139). At this time, the infant Rhesus was only 1 month old, so that it is difficult to see how its sensorimotor co-ordinations could possibly allow of imitation in the way they are using the term. Nevertheless, these comments do point to some interesting possibilities. The first, and probably basic, fact is that the mother's movements sometimes lead to the infant attending to certain objects—probably approaching them and touching or mouthing them. The second is that fear is apparently aroused in the infant by the mother's antagonistic behaviour, but possibly this has no bearing upon a conditioned social reaction, for the infant's fear could have been directly caused by the *inattention* of the mother to the infant.

Tinklepaugh & Hartman (1930) make some observations on 11 parturitions in Rhesus monkeys some of which are suggestive of ways in which feeding habits may be acquired by the infant. Thus, the yearling son of one female '...participated with his mother in eating a small part of her afterbirth. Though this limited evidence would in no case justify conclusions, imitation may be offered as a possible explanation of the behaviour of the male. Frequently monkeys and apes, when they see human beings or animals of their kind partake of it, eat food which ordinarily they would not touch. The young monkey...during a series of observations was caged with a turtle as his sole companion. Offered raw beef or liver, he refused it, but when the turtle approached this food and began to eat, the monkey rushed up, seized the meat, and eventually chewed and swallowed it' (p. 90).

Mitchell (1922) was probably the first person to produce reliable evidence on the discrepancy between the reactions of adult and infant apes to 'noxious' objects, such as live snakes. He had hoped to test out the differences on an infant monkey or ape born and raised in a Zoo. Failing this, however, he studied a very small baby chimp not much more than weaned when it came to the Zoo. He took a very active tree-boa and placed it in a box near to the chimp, who leaned out towards it. He allowed the flicking tongue of the snake actually to touch the chimp's face. The chimp '...showed no sign of alarm and was quite ready to kiss the snake's mouth. It was clear that, in this case, no particle of dread of the serpent was present. Since then, the chimp has on several occasions been shown snakes, including a large King snake. The latter he handles quite freely, plays with it and pulls it about' (p. 348). The author says that, although this may be a case of 'instinct failure', it is more likely that fear of snakes as characteristically seen in adult chimps is '...an acquisition due to experience or the imitation of other chimps which have had individual experience' (p. 348).

Jacobsen, Jacobsen & Yoshioka (1932) report similar observations on a chimp during her first year of life, these being in marked contrast to the fear responses shown by adult chimps. After presenting the infant with a number of neutral objects, such as a bottle and an electric bulb, with which she played, a piece of soft rubber tubing was put on the floor near her. Though showing no definite fear, her response '...was notably different from that made to other objects in that she approached it cautiously, retreated, and approached again' (p. 66). A rather sluggish live puff-adder was then placed near her. At first, she gave it no special attention, but: 'After a few seconds, she approached the snake and struck it several times with the back of her hand as chimps very commonly react to small living objects such as insects or rodents. The snake, thus disturbed, responded by puffing and hissing pronouncedly. She gave no visible attention to either of these reactions, nor did she definitely avoid the snake' (p. 66). The authors stress that this marked difference between the reactions of the young chimp and those of the older animals '... emphasizes the importance of an undetermined factor, either maturational or experiential, in the development of this interesting snake–primate relationship...' (p. 67). It must be stated, however, that none of the observations quoted made any attempt to control for the possible influence of the human observer's attitudes upon the responses of the experimental object. Probably this factor is not important, but a control of it is necessary in further work.

Nissen (1944) indicates that the characteristic nest-building behaviour of chimps in the wild is something in which early life experience plays an important part, for: 'Our nursery-raised chimps...never construct nests, no matter how favourable the opportunity. Unlike birds, in whom nest-building is an instinct, this activity is evidently a cultural acquisition among chimps, something that the younger generation learns from its elders. Where such educational opportunities are lacking, the custom drops out' (no page reference). Yerkes (1943) gives details of this in describing how five young captive-born chimps made no attempt to build nests when roaming free on a New Hampshire farm, though one started what looked like nest construction but never completed it, while Bernstein (1962) has confirmed that all wild-born but only a few captive-born chimps build nests when a variety of materials is made available to them. Harrisson (1960) likewise suggests that all young orangs, once introduced to nest-building by others, build nests in trees or inside their cages, thus implying that this kind of performance requires at least something of imitative learning for its full form. The mere tendency to handle and pull about sticks and branches, seen in all the young animals, needs something in the example of the experienced elders for it to be directed to actual nest construction.

Harrisson (1960) is clear also that the young orangs she kept in captivity learn something of their feeding habits by observing their mothers and other orangs. Thus, one infant did not like bananas, and only took to eating them after observing another young one eating them with gusto. One orang, about 4 years old, watched intently when workmen lifted a heavy cement cover of a sewage tank in order to clean it. The following day when this orang was let out, it went straight to the sewage tank and tried to lift the lid. He had never before taken the slightest notice of the lid and this observation shows how the active exploratory tendency and curiosity of the young animal may, through example, be directed to a new object, though the object has never been associated in its experience with, say, food or other 'primary' reward.

These informal naturalistic observations on captive young monkeys and apes are very far from negligible in their implications for the importance of learning by observing. As with the field-study data to be reviewed later, it is, however, difficult to disentangle all the possible factors that may be operating. Focusing of attention is clearly the major factor, but it is also obvious that this is done almost always in the process of going near to and closely watching an animal with whom the watcher is on positively affectional terms. Thus, Yerkes (1943) says regarding the learning of feeding habits by the infant chimp: 'It greatly facilitates acceptance if the mother or other experienced individual sets an example of acceptance or avoidance, or actually offers the substance to the infant' (p. 119).

The best-known studies in which imitation is reported are those of Kellogg & Kellogg (1933) and Hayes & Hayes (1951, 1952, 1953). In the former study, the young female chimp, Gua, was brought up for 9 months with the authors' son, Donald, who was 2½ months older than her. Their observations chiefly demonstrate the superiority of Donald's powers of imitation over those of Gua. Thus: 'Probably the most convincing proof of the child's superiority in imitation was his direct mimicry of the ape herself' (p. 141). Imitative play was a feature of the behaviour of both, and the ape often followed the child's example, as when he went to a small radio bench and hammered its seat with his hands to produce a metallic drumming sound. 'She would also open cabinets and cupboards, probably as a result of her observation of others, and during the later months she needed no encouragement to put a hairbrush to her head and make brushing movements like the child' (p. 139). Particularly in vocal imitation (of her grunts and barks) the child was obviously superior, and there seems no evidence of any reciprocal imitation of this kind on the chimp's part.

The subject of the Hayes studies was a female chimp, Viki, adopted by them a few days after her birth, and brought up in their home to all intents and purposes like an adopted human baby. Their informal observations showed (1951) that Viki could imitate in response to the command 'do this' by copying the action which they demonstrated. This technique was useful in teaching her to learn new skills and the appropriate manipulation of objects with which she had not previously done more than play. The more formal problem-solving experiments on imitation (1952), using stick-and-tunnel, ball-throwing, stick-and-string tasks, and so on, led the authors to conclude that she was clearly able to profit by their demonstrations of correct solution in a way that could not be attributed merely to trial-and-error and insight-learning. They compared her successes with the relatively poor results achieved by another cage-raised chimp on the same tasks. Further, her 'imitative play' seems to have included acts of genuine copying, while her success on a delayed imitation task was particularly impressive: 'Viki was allowed to watch us use a screwdriver to pry off the friction-type lids of several empty paint-cans. She was then taken away and brought back one hour later. She set to work immediately, and after some effort succeeded in opening two cans. (She was already familiar with various other containers and had often used a screwdriver as a lever, but this combination was new to her)' (p. 451). They emphasize that Viki had had no opportunity to acquire the necessary technique by trial-and-error, or by reward for chance performance. They also tested her (1953) on an 'imitation set series' in which she was shown, in order of testing, the following representations of humans and of Viki herself clapping hands, patting the head and protruding the tongue: (1) motion pictures, (2) projected stills, (3) photographic prints, (4) simple line-drawings. She performed fairly well in response to these, transferred readily through the various stages from movies to line-drawings, and would often work without being rewarded for correct responses, treating the tests as a game. However, although 'she seldom failed to imitate preferred actions (those which she often performs spontaneously, in play),...she often made errors by performing such preferred actions in response to pictures of non-preferred items' (p. 471).

The Viki observations are convincing, and give detailed support to the generalization of Yerkes & Yerkes (1929): 'We feel entirely safe in asserting, as the consensus of opinion among competent students, that the chimpanzee commonly and with extreme facility imitates acts, in some instances for the mere satisfaction of performing them, and in other cases for the sake of a desired reward or objective' (p. 347). Nevertheless, it is still too soon to be able to state explicitly, on the basis of this kind of evidence or that of the formal laboratory kind, exactly what weight should be given in such performances to the several factors involved. These factors clearly include a spontaneous (imitative) play tendency, social facilitation, and processes of learning which may still turn out to differ from trial-and-error, classical conditioning, and insight, in the important fact that they are embedded in a strongly positive affectional background of social relationships.

Captivity studies of adult animals

Observations of imitation by mature monkeys and apes are probably of less significance than those which have just been reviewed. Nevertheless, as the Yerkes & Yerkes (1929) quotation says, there is strong observational support, at least on anthropoid apes in captivity, that mature animals are, under favourable conditions, able readily to learn by imitating the performances of another adult of the species or of a human demonstrator.

Most of the work has been done on orangs and chimps, but Carpenter (1937) gives some data on two mountain gorillas in the San Diego Zoo, aged about 8 years. He says: 'There seemed to be considerable imitator–imitatee relationships between the two gorillas when their moods and behaviour were properly synchronized. However, the problem is raised of what is meant by imitated behaviour. Acts which are merely imitated by the activity of another animal are not acts of imitation' (p. 191); thus, '...simple forms of behaviour such as playing with objects, swinging ropes or rings, are copied from one animal by another', but these acts are already a part of the action system of the second animal (the copier). However, as he points out: 'When the behaviour of one animal stimulates similar behaviour in another, there is shown an interesting and important type of social behaviour that deserves further study, be it imitation or a more simple form of activity' (p. 191). Yerkes & Yerkes (1929) report consistent failures in teaching a female gorilla to imitate on padlock-hasp and other instrumental tasks, although she did learn, apparently by following Yerkes' example, to acquire more extensive feeding habits.

Yerkes & Yerkes (1929) cite numerous examples of imitation in orangs and chimps. Captive orangs have frequently been observed to imitate each other and also the human uses of various objects, but they more readily imitate what is seen than what is heard. The orang Julius had failed, after abundant opportunity, to solve the two-box stacking problem, so Yerkes demonstrated the correct placing of the boxes beneath the suspended banana, and then climbed upon them and reached it. Julius immediately climbed on to the boxes and obtained the food. In other words, the orang did not himself solve the problem, but merely took advantage of the correct arrangement left for him. In fact, Julius failed to do the task correctly on the next three successive days, and, on the fifth and sixth days, he was again given the demonstration. Only after this third demonstration did Julius suddenly succeed in doing the task for himself.

There is nothing to indicate in these observations that Julius had learned from the demonstrations any more than he might have learned from an equal number of unaided trials. The most that might be inferred is that his interest was renewed, following his own repeated failures, and that he had learned to place the boxes for himself after seeing the correct arrangement. This might have been achieved without Yerkes himself, or anyone else, playing the part of demonstrator. Their conclusion that '...definitely purposive imitation plays a highly important role in the adaptive life of this ape' (p. 176) needs much further supporting evidence from controlled observation and experiment.

Far the most comprehensive observations from the Orange Park laboratories concern imitation in chimps (Yerkes & Yerkes, 1929; Yerkes, 1934, 1943). The chimp is deemed to be easily the readiest imitator of the three great apes: '...in the order gorilla, orang-utan, chimpanzee, the great apes provide evidence of increasing frequency, complexity, and serviceableness of imitative behaviour' (p. 574, Yerkes & Yerkes, 1929), and: 'the level of social development in the chimpanzee may be gauged also by its degree of suggestibility and imitativeness' (p. 51, Yerkes, 1943). The observations relied upon are, however, almost entirely informal and non-experimental. Thus: 'Literally scores of times I have seen our [chimpanzee] subjects acquire useful acts by watching apes or men. Examples...are most frequent in connexion with eating and drinking and the use or adaptation of objects as tools' (p. 142, Yerkes, 1943).

Yerkes & Yerkes (1929) clearly recognize the complexity of what goes under the general term 'imitation' in pointing out at least three phases of behaviour in chimps: (1) attraction to and intent observation of the behaviour of its fellow; (2) this may lead to near approach to its fellow and effort to keep near it; (3) sometimes this may lead to '...prompt and obvious reproduction of the act observed, the one organism being stimulated by observation of the other to do the same sort of thing or to achieve the same result' (p. 345). Such distinctions in the phases of the social behaviour involved in the usual imitation situation, whether experimentally set or informally observed, should, together with others that could be equally clearly defined, be more frequently used, for they may well illustrate more precisely what goes on than the usual simple criterion of a scorable difference in speed or accuracy of imitative versus non-imitative performance.

The nearest approach to actual experimental controls of chimp imitation is Yerkes's

(1934) study of suggestibility, in which he used twenty-one subjects. The experimenter (*E*) first presented on 4 successive days to one chimp, the others having the opportunity to look on, pieces of filter-paper just prior to the mid-day feed. On the next 4 days, *E* took the piece of paper, put it in his mouth, and chewed it, while the chimp was watching. Broadly, the results showed that some chimps evinced a marked interest in the actions of the *E*, but that, thereafter, their attention wandered. Response to the suggestion seemed greatest in the 10-year-old animals, less in the 5- and 15-year-old ones, and varied also with the social status of the individual. None of the chimps attempted to chew or swallow the paper in the way demonstrated by *E*.

There is thus nothing precisely verified under controlled conditions nor fully analysed in the repertoire of imitativeness described for these animals, and, further, it follows that comparisons between the anthropoids are prematurely made in that the tasks set and the circumstances of the observations vary so much. While it would be unreasonable to doubt that these animals do gain much from observing the behaviour of their fellows, and of human beings, the extent to which the modifications are due to social facilitation, to localizing, and perhaps to accentuation of play and exploratory tendencies, as much as to the learning of additional skills, is difficult to determine. Probably, as the Hayes studies indicate, facility and readiness in this respect is cumulatively brought about in the life experience of the individual, with progressive transfer from early habits accounting for the more dramatic examples sometimes recorded in the adult animal. It is further of great importance to try to discover the circumstances of the natural social group within which the infant is brought up, for such knowledge may have a vital bearing upon the contribution of imitative learning or even tuition to the mature animals' behaviour. Thus, Bingham (1928) says: 'It is not improbable that imitation in a chimpanzee group is a potent factor in the ontogenesis of copulatory efficiency' (p. 37), and this is supported by Morris-Goodall's (personal communication) field observations, which may also do much to establish how the other essential habits, of feeding, avoidance of noxious objects, and so on, are influenced in the wild.

Köhler's (1925, 1926) observations on imitation in chimpanzees add little to those already examined. He believes that imitation, in the sense of an intelligent and insightful perception of the significance of a demonstrator's solution of a problem, is not particularly common in chimps: 'Imitation is almost as difficult for apes and as rare in them as it is in lower vertebrates. One does observe imitation of different forms or types in apes, but not so very often, and only after certain conditions are fulfilled' (p. 156, 1926). He goes on to describe the 'serious play' type of imitation in children, and gives examples of this in chimpanzees, as where one observed a man paint a pot white, leaving behind a brush and a pot of white paint. The chimp soon took the brush, put it into the pot, and painted a big stone white. 'The whole time the ape behaved completely seriously. So did others when imitating the washing of laundry or the use of a borer' (p. 157). Something more than serious play was said to be involved when a chimp that had failed repeatedly on a box-stacking problem responded immediately and successfully after Köhler had himself demonstrated the correct solution. This sort of imitation is said to involve essentially the perception of a new set of relationships which are, for the first time, demonstrated to the watching animal, and it thus facilitates the reorganization of the parts of a problem when the trial-and-error efforts of the animal had led to repeated failure. While such imitated solutions are said to be difficult for the chimp, these animals will imitate with ease if they are already familiar with and understand the action to be imitated.

On monkeys, there are scarcely any naturalistic observations of imitative behaviour, but Corner's (1955) interesting account of the training of Pig-tailed macaques to aid him in botanical collections of twigs, flowers, leaves, fungi, etc., in the Malayan forests certainly suggests that the training programme is aided by human demonstration. He further describes how a young macaque, taken from its mother's breast in the wild, gradually learned what to eat in the forest by watching an experienced monkey.

Field observations

The evidence on imitation in wild groups of monkeys and apes is limited by the fact that the observations must be made at so close a distance from the animals as to be unambiguous of interpretation. Carpenter (1934) reports that Howler mothers control and condition their infants by facilitating their 'spontaneous' activity at times, and on other occasions by restricting it. An example of facilitation is where the female '...pulled off a terminal twig filled with buds, and after eating part of them, gave the rest to a small infant which was associated with her. The infant nibbled at the food' (p. 74). One suspects that this example comprises little, if anything, more than the focusing of attention of the infant on what is appropriate for eating, rather than anything in the way of 'tuition'.

Hall (1962), in making very close-range observations of a wild Chacma baboon group, has frequently observed how dark-phase infants, prior to their actually eating food-plants, watch the feeding of their mothers or of other mature animals near them, and tend to direct their exploratory movements upon the same plants, chewing them or putting parts of them in their mouths. Older juveniles following, for example, feeding adult females have stopped to dig for bulbs in exactly the same place (film of the feeding behaviour of the Chacma baboon, photographed by S. Peet, and directed by the writer). The extensive range of food plants used by these animals are located and treated in many different ways by the group. Some edible bulbs can be located by only a tiny outgrowth above the surface of the ground. These have to be dug out. Others can be pulled from the soil by a tug on the leaves. Of some plants, such as the wild geranium, only the stems are sucked, the leaves being ignored, and so on. One may suppose that the group feeding-pattern, at least in the details of discriminations and manipulations, is largely acquired through the accumulations of daily observational learning. Some corroboration of this came when we released a tame baboon in the vicinity of a wild group. The tame animal came from North Transvaal, and had been in captivity, from about 6 months after its birth, for 3½ years in the Cape. It was thus unlikely to have had the opportunity to acquire many, if any, of the feeding habits of the local Cape group. In the course of several days following its release, it came to be accepted, through a series of greetings episodes involving mutual embracing, kissing, and so on, by most of the animals in the group. As it came thus to be accepted, it spent much of the day feeding close to the others, and could sometimes be seen sitting very close to one of the others as the latter dug in the ground for bulbs. It then dug in the same place.

Chacma baboon groups vary, of necessity, quite markedly in their feeding habits in different parts of their distribution. In South-West Africa, a recently captured young male readily took and ate live scorpions. In the Cape, each of twenty different baboons of a group reacted with avoidance or hitting away when discovering live scorpions given to them experimentally under stones or in paper bags which they had learned to associate with desirable foods such as sweets (Hall, 1963). In South-West Africa, the same young baboon readily ate live legless lizards, while, in the Cape, the wild baboons reacted with fear to similar creatures. One could multiply the examples of these studies which strongly indicate that at least some of the local feeding habits and aversions are conditioned in some way, probably through observational learning, by social example.

The work of the Japanese field-studies on *Macaca fuscata* groups observed over very long periods, and often at close range (Imanishi, 1957, 1960; Frisch, 1959; Reports of the Primates Research Group, 1955), not only indicates how the spread of a new feeding habit occurs in a group, but describes how social status may itself be in part determined by the status in the group of the animal's mother. Thus, it is suggested, the attitudes of the individual *vis-à-vis* others in the group are, to some extent at least, taken over (i.e. imitated) from observing the mother's behaviour in the group and the behaviour of the group to the mother. According to Imanishi (1960): 'Children of dominant females involuntarily learn attitudes of the dominant and those of submissive females learn attitudes of the submissive. Moreover, children of dominant females in the central portion [of the group] are more intimately related to the leader than those of submissive females in the peripheral portion, as their mothers are more intimately related to the leader' (p. 3). Further: 'If other things are equal, males growing up with successful identification will co-operate with leaders more willingly, be accepted by them as well as by females more easily, and finally succeed their leaders...' (Imanishi, 1957, p. 53).

Such field observations upon the social matrix in which basic emotional attitudes relating to dominance and submissiveness are acquired within a wild group are of the greatest interest, for they indicate, perhaps for the first time in non-human primate studies of any kind, that the processes of imitation, in its broadest and most realistic sense of the social modification of individual behaviour, need to be studied not merely in the problem-type of skill acquisition experiment but in the simple setting of social variation.

The spread of a new feeding habit is likewise graphically described in these Japanese studies. Thus, a habit of washing sweet potatoes in a shallow stream before eating them was started by a female aged 3 years, then was transmitted to two infants of 1 and 2 years, and one of these infants transmitted the habit to its mother, then, later, altogether eleven other members of the group acquired the habit (Primates Research Group, 1955; Imanishi, 1957). Similarly, some juveniles of a group picked up the habit of eating caramels, when these were offered by visitors, and passed it on to their playmates. From the children the habit spread first to the mothers, then to the dominant males, while the last to pick up the habit were the subadult males who are furthest removed from and without contact with the members of the centre of the group (Frisch, 1959). In this group, the spread of the new habit took over 3 years. Although, as Imanishi (1957) states, infants acquire a new feeding habit very readily, the adults are usually rather slow in picking it up, but, as already stated, mothers quite often learn to eat a new food by observing their own infants, as well as the reverse.

The importance of these field observations lies in indicating a great variety of new experimental approaches to the problem of imitative learning in monkeys and apes. We are faced with unequivocal evidence that certain fundamental social attitudes, feeding habits, and aversions, are acquired by the members of a group, and that the habit patterns or 'traditions' of groups vary to some extent from one group to another of the same species and even within the same population area. As Zuckerman (1932) says: 'Living as they do in permanent social relationships, they [monkeys and apes] are provided with numerous opportunities for modifying their behaviour through imitation' (p. 165). Field and laboratory experiment should now be devoted to testing far beyond the narrow framework of the traditional imitation study the nature of the affectional and social factors that make for, or inhibit, imitation. Care in technique, method, and criteria definition, while valuable accessories, should now be imaginatively used on the realistic social learning problems of these animals.

CONCLUDING COMMENTS

Looking back over the evidence, the experimental approach has been mainly concerned with demonstrating, or not demonstrating, that imitation learning occurs in adult monkeys and apes, keeping some factors constant, but often not the important ones. Naturalistic observations and informal studies, such as most of the early ones, usually indicate that observing the acts of a *D*, whether human or of the same species as the *O*, leads to a change in the behaviour of the *O*, which it seems is unlikely to have occurred without the action of the *D*. However, in all types of study, controls of previous experience or a sufficient knowledge of it are often lacking, and it is hard to tell whether the increment in performance was due to something specific which the *O* had seen or whether it was caused by a simple accentuation of interest or a change of 'set', i.e. a distraction away from an inadequate trial-and-error approach, which might conceivably have been achieved by any of several other kinds of diversion.

Developmental observations, scanty though they are, have been particularly suggestive, for they strongly indicate that, in the affectional context provided by the mother and others of a species group, places or objects are discriminated as significant rather than neutral by 'following' the actions of the *D* animal, but not necessarily by 'copying' the motor patterns of the *D*. Avoidance-habits and feeding-habits may well be largely determined in this way, as also may some of the attitudes of preference, aversion, dominance or submissiveness towards other members of the group.

It is perfectly reasonable to suppose that there is a highly formative period in the early years of life of the juvenile monkey and ape during which avoidance and other importantly transferable general social habits may be very readily acquired, but one may doubt whether, in these animals, there is any evidence of a critical physiological period during which alone certain fundamental attitudes are acquired through social observing. It seems likely that, because of the 'natural' affectional context in which the young of these animals, and perhaps of all mammals, develop, a tendency to learn by observing others is characteristic of all young monkeys and apes unless there is prolonged isolation from the species stimulation more or less from birth or weaning onwards. Probably this tendency is determined during the period of mother-dependence, and then, normally, carries over in the process of gradual independence when the young associate more and more with others of their own play-age group. One must also take into account the fact that the normal young primate is a highly exploratory animal, and that its explorations go far beyond a mere tinkering with all or any objects, for it tends selectively to explore what others explore, and tends also, in the same sort of way, to learn appropriate attitudes towards others in the group, including the dominant animals.

What, then, is involved in the various forms of observational learning? As Thorpe (1956) has pointed out, there is plenty of evidence for social facilitation and local enhancement in mammalian behaviour, but the evidence for 'true imitation, in the sense of copying a novel or otherwise improbable act—some act for which there is clearly no instinctive tendency' (p. 411)—is hard to discern. However, 'true', or what used to be called 'intelligent' or 'inferential', imitation is only one aspect of what might be considered a continuum of conformity processes going on in the natural life of monkey and ape. Indeed, it would seem more interesting scientifically to pay much less attention in experimental studies of these animals to the possibilities of human-like performance—involving 'awareness' of self and of another self—and much more attention to the ontogeny of simple habits of avoidance, social attitudes, and so on within the setting of the natural or experimental group. There seem likely to be several profound questions here for developmental research which are obscured by the fact of their 'ordinariness' or 'naturalness'. In considering, for instance, the cohesive forces that keep together year after year a group of wild monkeys, it is evident that far more is involved than is comprehended even in the broadest definition of sexual motivation. The dynamics of the group processes can best be studied by means of experimental manipulations in the zoo park or even in the natural situation.

It would be premature to try to discuss the comparative extent and quality of imitation in the monkeys and apes and man, for there is too little reliable experimental or naturalistic evidence. But it is reasonable to suppose that a profound analysis of the conformity processes in the non-human primates will be of significance for our understanding of the origins of basic social attitudes in the human child, at least in providing new hypotheses for investigation which might not so easily emerge from the accustomed observations of the children themselves. Indeed, it would be useful to concentrate upon the acquisition of attitudes that are *not* verbally associated and transmitted, leaving aside the obvious differences between monkeys, apes and man in the use of symbols.

Finally, there seems no point in trying to sort out the precise relationships amongst the several conformity processes evident in this review to the standard learning processes of habituation, conditioning types I and II, insight, and so on. For, in a real sense, the processes under discussion cut across these forms, any one or more of which might be involved in the behaviour sequences. And, further, we must be primarily concerned with the ontogeny of social learning, not with a cross-section of that which the adult representatives of any particular species can be made to achieve on a sample of imitation tasks. The apparent confusion of terms referred to earlier on in this paper may not be so much the outcome of semantic inefficiency as a real indication of the complexity and variety of the conformity processes. What is now needed is a sustained observational and experimental attack designed, not at first to reduce the processes to simple order, but to bring clearly into evidence the 'non-intellectual' factors that seem to be crucially important.

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THE WORD-IN-CONTEXT: A NEW TYPE OF VERBAL REASONING TEST

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A description is given of a new test of verbal reasoning which is used in a small-scale experiment. The novelty of the test derives from its 'real life' material, its appraisal of induction as well as deduction and its provision for the objective scoring of different degrees of correctness. In addition to the subject's final solution to each problem, the test provides evidence of how this decision is reached.

The experimental results suggest (*a*) that the Word-in-context does effect discrimination among intelligent young adults, (*b*) that the cognitive qualities on which it discriminates are not the usual qualities assessed by cognitive tests, (*c*) that it is possible to produce an objectively scorable test in which arts students do as well as science students, (*d*) that what is tested, though strongly verbal in bias, is not vocabulary strength. It is hoped that the test may prove useful for purposes of high-grade selection, as an indicator of certain aspects of personality and for the study of concept-formation and other thought processes.

I. INTRODUCTION

The traditional type of paper and pencil intelligence test has now been in existence for over 40 years and it has flourished. It shows little sign of the ills that middle-age is heir to, apart from increasing narrowness and rigidity, and some tendency towards ingrowing. Yet these symptoms, though regarded by many as desirable, may develop into serious obstacles to healthy maturity.

Intelligence tests have, during the last two or three decades, become 'purer', more and more artificial, less and less concerned with matching themselves against external criteria. The statement, for example, of Vernon (1960), in a section *on test validity*, that 'factor analysis is still the chief tool employed in determining how far a test measures g or v', is representative of most current thinking on this topic. The resultant purity and artificiality of tests is regrettable, both from the viewpoint of motivation in the subject (especially the adult subject) and from that of validation. To use factor analysis as a means of 'validating' a test is to travesty what used to be a clear and useful term.

Intelligence tests are now pure, in both the factor analysis sense and the item analysis sense. This results often in a test consisting exclusively of one type of medium and, within that medium, of only one type of principle. This leads to monotony and artificiality: the task of the subject (who tends to be forgotten) has over the years become divorced to an ever greater extent from his activities in a non-laboratory, non-test situation.

A second, practical, corollary of factor analysis theory is the ever-increasing isolation of the cognitive qualities, as tested, from the orectic qualities. To use contemporary terminology: g is not only separated from w, n, etc., but also from X ('industriousness factor'). The more successfully a new test segregates general intelligence from 'irrelevant' temperament traits, such as obsessiveness, conscientiousness or self-confidence, the better the test is deemed to be. Yet school-teachers and university tutors insist that they are interested in such traits, no less than in intelligence; and even some psychologists are aware that an obsessional subject, for instance, presented with a standard intelligence test may do less well on it than a more happy-go-lucky subject of equal, or even of lower, intelligence. It is true that certain existing tests—notably projective techniques such as the Rorschach—aim to assess both temperament and intellectual calibre. But, apart from the fact that these tests are time-consuming to give and subjective in interpretation, the notions of *correctness* and *incorrectness* of response scarcely apply. Such tests, therefore, convey only a general idea of the subject's intelligence level: they cannot yield much that is quantifiable or usable for purposes of comparison.

It is with these strictures in mind that the Word-in-context (Heim & Watts, 1958) was conceived. It is intended to be primarily a test of verbal reasoning, but it is believed to differ from other such tests, in the following respects:

(*a*) It provides a test situation which is similar to, if not identical with, a frequently-met real life situation.

(*b*) It requires inductive reasoning from the subject, in addition to deductive reasoning.

(*c*) It is objectively scorable in the sense that a correct answer exists for each problem. It has the advantages, however, of being 'creative answer' in form and also of allowing for *degrees* of rightness (and wrongness).

(*d*) It does not seek to isolate the cognitive aspects, in the way that has become customary. In addition to its assessment of reasoning ability the test may yield data concerning certain non-cognitive traits, such as flexibility and conscientiousness.

(*e*) There are grounds for hoping that the Word-in-context may prove to be a useful tool in research on thought processes, in particular on concept formation, and also in clinical work with older people and deteriorated patients.

II. The Tests

A. *The Word-in-context*

The Word-in-context test is in two parts. First the subject is given a list of words, each of which he is asked to mark as A or B or C. A signifies: 'I know this word and could explain it to someone unfamiliar with it.' B is a broad category indicating doubt. C signifies: 'I have never seen this word before and have no idea what it means.' If a subject raises difficulties or demands an intermediate category, he is reminded: 'When in doubt, use B.'

Having marked the words A, B or C, the subject is given a set of contexts for the words he categorized as C. Each set consists of three typed contexts (one context per sheet), presented in *descending* order of difficulty. The subject reads the first context (P)—the most difficult of the three—and writes down the meaning of the word, in so far as he can interpret it from its context. He then does the same for the second (Q), easier and for the third (R), easiest, context. He may interpret the word on its second appearance in the light of his present and previous evidence; and on its third appearance he has the benefit of the preceding two contexts. At the end, he may reread all three passages and then he is asked to give a final verdict on the meaning of the word. It is the 'final verdict' scores which are to be used as the test of verbal reasoning. The rationale of calling this process 'inductive' should now be clearer. It is hoped, too, that the answers to the separate Word-in-contexts, *P*, *Q* and *R*, may yield data concerning such temperament traits as flexibility, conscientiousness, and willingness to commit oneself on inadequate data.

The words in the list are selected from English and American prose, by reputable authors. Many of the words, however, are obscure, archaic, dialect, slang or of foreign origin. An effort is made to ensure that all the words in the passages, apart from the crucial test word, are well known to the subject. Each of the three passages is culled from a different writer. Below each passage is given the name of the author, the publisher, the book or periodical and also the date of publication. The original works are for the most part novels, biography, travel or articles on current affairs. Technical and scientific writing is excluded, as is poetry or translation. The average length per context is a quarter to a third of a quarto sheet of typescript and the crucial word usually occurs about the middle of the context (see Appendix I).

The final verdict is scored on an 8-point scale. The three-context sets have proved difficult enough to yield a wide variety of final verdicts, even from university students. There is, however, some uniformity among the diversity, thus enabling a reasonably consistent system of scoring to emerge. This ranges from -2, the score for a wholly mistaken interpretation, to $+5$, an interpretation which is correct and complete. See Appendix II for an illustration of the scoring.

B. *Other tests used in the experiment*

Two other tests were used in the experiment:

(1) The Self-judging Vocabulary test (Heim & Watts, 1961).

(2) The Pattern Perception test (Penrose, 1944, 1946).

The Self-judging Vocabulary test is a standardized, difficult test of vocabulary, known to effect differentiation among university students. It consists of 80 words. These are presented, alphabetically, in a list whose items the subject is required to mark *A*, *B* or *C*, in the same way as at the beginning of the Word-in-content test. The marked list is then removed and the subject is given a test booklet in which the same 80 words appear, each of these being followed by six short phrases. The subject is told that in each case one of the six alternatives corresponds to the correct meaning of the word and he is asked to put a ring round the number of the correct phrase. He is instructed to guess when he does not know, and to give a seventh answer in his own words in cases where the crucial word is known to him and he dislikes all the six alternatives offered. No time limit is imposed.

The Pattern Perception is a high-grade general intelligence test with a diagrammatic bias. It comprises 64 questions all of the 'classification' type. Each item consists of five figures of which four have some common feature that the fifth lacks; or of two pairs—again leaving the fifth as 'odd-man out'. It was designed for students and does effect some discrimination among them. The test has a 30-minute time limit.

III. SUBJECTS AND PROCEDURE

The subjects were forty students from Cambridge University. Of these twenty were reading Arts and twenty were reading Science (including six mathematicians). Within each subgroup, half were men and half were women. There were thus ten male arts students and ten female arts students, ten male scientists and ten female scientists. Their arts/science allegiance was determined by the Part I Tripos which they had taken or were proposing to take. Thus, for example, a student now reading Psychology Part II (part of the Natural Sciences Tripos) who had read English for Part I was classed as an arts man. This decision was made in order to avoid the necessity of a third group of 'mixed' arts-plus-science subjects. All were British and under 25 years of age.

Subjects were required to come on two occasions. The first time, they took the Word-in-context test, as described above. The *A*/*B*/*C* list consisted of 35 words and quite good agreement was found as to which were *C*-words, i.e. quite unknown, thus allowing most of the subjects to do many of the same Words-in-context. After having finished with the *A*/*B*/*C* sheet, the subject was allowed 45 min. in which to do as many Words-in-context as he could. A limit on time, as opposed to number of Words-in-context, was set for administrative reasons. In the event this proved expedient for a few subjects were very slow and would evidently have taken about 1¾ hr. to do as many sets as others did in 45 min.

On his second visit, the subject took the Self-judging Vocabulary test and the Pattern Perception test.

IV. AIMS

Two major criticisms have been made of the Word-in-context in its early stages: (*a*) that it is both time-consuming to collect, and unnecessary to use, 'real words' in 'real contexts'. 'Why not make up the words and/or the passages, yourself?' is a frequent question. (*b*) That the Word-in-context is 'obviously' just a test of word knowledge (despite the fact that for all subjects only '*C*' words are used). Therefore 'would it not be simpler to use a straightforward test of vocabulary?' The present experiment was designed partly in order to meet objection (*b*) and the answer may be found in the results set out below.

As regards question (*a*): it would clearly have been less lengthy a process to manufacture our own words and contexts, but we believe that the resultant test would have been less valuable, for the following reasons.

Real words

(1) The attempts of other psychologists to make up unexceptionable and convincing words have not been conspicuously successful. See, for example, the experiment of Werner (1954), who made up his words (and also his sentences). He devised 12 words, such as 'contavish', 'lidber' and 'prignatus'; and six sentences for each word. For example: '(i) Boys sometimes prignatus their parents. (ii) Mary did not know that Jane used to prignatus. (iii) Mother said, "Jimmy, you should never prignatus your own mother".' And so on, up to no. (vi): 'If Bob prignatus somebody he makes sure they don't find out.' The meanings that Werner intended for the three words were, respectively, 'hole', 'gather' and 'deceive'. But in our view these examples, which are representative of the whole test, demonstrate the misleadingness of the artificial words and the inevitable poverty of the *ad hoc* contexts. Subjects are differentially helped or hindered by the sound, look or form of the artificial word. 'Real words' have roots and grow up rather as a tree than as a laboratory brass instrument and this affects most of us in our meeting and use of new words.

(2) It has been demonstrated (e.g. Glaze, 1928; Conrad, 1962) that even the simplest and briefest of nonsense syllables do have association value. Thus the original reason for using nonsense syllables (in other experimental situations) does not hold. There are, in fact, serious objections, which seem to us to be still more serious when the test concerns word-meaning than when it concerns memorizing.

(3) In view of these objections to artificial words, the use of a space or dash has been advocated by those who wish to avoid 'real words' at all costs. There are two objections to this. First, a space or a dash breaks up the text. In normal reading, one quite often meets an unfamiliar word (sometimes without realizing it) and, after several such meetings, the word becomes assimilated into one's vocabulary, whereas an obvious gap would bring one up short. Secondly, an unpublished experiment by K. Briggs* shows that the results, on subjects equated for vocabulary strength, do in fact vary according to whether the unknown in a context is (i) a dash, (ii) an artificial word, or (iii) a real word. Since different results are obtained, and since our aim is to reduce as far as possible the element of artificiality in the test, real words are evidently preferable to spaces or dashes.

(4) In the Word-in-context, the crucial word is the one which was actually chosen by the original authors of the passages. They presumably selected it on account of its look, rhythm and flavour, in addition to its meaning. As would be expected, the same word has different *nuances* for different writers. Hence our insistence that the three contexts comprising a set should come from three different authors. This important aspect of living language would be lost if anything but real words were used.

Real contexts, by reputable authors

(1) Point 4 above applies to the contexts as well as to the individual words. Variety of matter and style is assured in a way which would be impossible if the experimenters had been responsible for the passages. The reader has only to look at the sentences produced by Werner (1954) and other similarly intrepid investigators to assure himself on this point.

(2) It is felt that the use of passages drawn from published books and articles renders the test situation less artificial. The technique used is merely an extension of the way in which everybody acquires new words from infancy onwards—by hearing or seeing unknown words, repeatedly, in different intelligible contexts.

(3) Interest and motivation are well maintained among varied types of subjects, some exclaiming 'It makes you want to read the whole book', some even noting the name of the book. The fact that George Eliot or P. G. Wodehouse has used the word in the given context also provides a convincing answer to the occasional critical subject who would otherwise wish to query the use of a given word in a context.

It was hoped by means of this experiment to obtain answers to the following questions:

(1) Is the Word-in-context in its present form a viable test, suitable for adults of high intelligence, i.e. is reasonably objective scoring of the final verdicts practicable and, if so, do the scores yield a sufficiently wide range? Data hitherto existed only on (*a*) naval ratings, most of whom had taken single Words-in-context, in order to determine the relative degrees of difficulty of the passages and (*b*) a group of women students who had *four* contexts per set. The latter experiment was useful in that it showed that inclusion of the fourth context—which was also the easiest—rendered the test too easy at this level. The majority of the final verdicts given by these women were correct, or too near-correct for discrimination to be effected.

(2) Is the Word-in-context testing something different from the usual high-grade

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* 'The effect of context on real words as compared with gaps or manufactured words' (1959).

test of general intelligence? One of its objects is to assess inductive as well as deductive reasoning; and to allow the subject who has correct 'hunches' (i.e. probably, the subject who is sensitive to implicit hints and reacts to very slight indications) to come into his own. It is also 'open-ended' as opposed to multiple-choice. This might well result in a *low* correlation between Word-in-context scores and scores on a traditional type of intelligence test. For this reason, the Pattern Perception test was included in the experiment.

(3) If the Word-in-context is assessing something other than that which is tested by traditional tests of intelligence, is this something merely vocabulary strength? For this reason, the Self-judging Vocabulary test was included in the experiment.

(4) Are there significant differences on the Word-in-context between scientists and mathematicians on one hand and arts people on the other? It may be remembered that scientists and particularly mathematicians have been found to do better on reasoning tests than arts people (Wallace, 1952). This finding has sometimes extended even to purely verbal intelligence tests (Dale, 1935).

(5) Does the Word-in-context yield differences between men's scores and women's scores, in respect of either mean or scatter?

V. RESULTS

Table 1 gives the mean, range and standard deviation of the three tests used. It may be recalled that the possible maximum score for Pattern Perception is 64 and that that for Self-judging Vocabulary is 80. It is less simple to determine the theoretical maximum for the Word-in-context, since as soon as the subject finished one set he was handed another, until the 45 min. had expired. Moreover, on the Word-in-context (unlike the other two tests) it is possible to obtain a negative score. The maximum mean-per-word is, as stated above, 5·0.

It is thought useful at this stage to keep a record of both the Word-in-context total score and the Word-in-context mean-per-word. The former to some extent indicates speed as well as power; the latter—being independent of the number of sets attempted—is an index of power only. Since the total score and mean-per-word are somewhat interdependent, the correlation of total score with mean-per-word is naturally high ($r = 0{\cdot}81$). In the text we shall therefore refer to the total score only but the results for mean-per-word, and also for number of sets of contexts attempted, are given in the tables.

An interesting finding in Table 1 is that the Word-in-context is the only one of the three tests which does not discriminate against either arts people or scientists, as such. On Pattern Perception, the scientists score significantly more highly than the arts students ($P < 0{\cdot}05$); and on 10 Vocabulary, the arts students score significantly more highly than the scientists ($P < 0{\cdot}02$). But on Word-in-context, whichever index be used, the difference between arts and science fails to reach significance level.

It is worth noting, moreover, that such differences as do obtain between arts and science, on the Word-in-context, are in favour of the arts student. Most tests of reasoning ability yield higher mean scores for the student reading science or mathematics than for the student reading arts. It has been argued that greater intelligence is required by the university science (or maths.) student than by the arts student, or that the type of intelligence which can be objectively assessed by tests is the type

Table 1. *Means, ranges and standard deviations*

Test		Total group, $n = 40$	Arts, $n = 20$	Science, $n = 20$	Men, $n = 20$	Women, $n = 20$
Word-in-context	Mean	17·2	18·4	16·0	16·3	18·1
Total score	Range	5–39	5–39	6–32	6–39	5–32
	S.D.	8·1	8·1	7·4	7·8	8·3
	Difference between means	—	(+)	(−)	(−)	(+)
			2·4, N.S.		1·8, N.S.	
Mean per word	Mean	2·39	2·45	2·33	2·19	2·58
	Range	0·8–3·9	0·8–3·9	1·2–3·6	1·2–3·9	0·8–3·8
	S.D.	0·79	0·84	0·75	0·75	0·80
	Difference between means	—	(+)	(−)	(−)	(+)
			0·12, N.S.		0·39, N.S.	
Number of sets attempted	Mean	7·07	7·50	6·65	7·30	6·85
	Range	4–12	5–12	4–10	5–11	4–12
	S.D.	2·01	2·11	1·77	1·71	2·22
	Difference between means	—	(+)	(−)	(+)	(−)
			0·85, N.S.		0·45, N.S.	
Self-judging Vocabulary	Mean	53·8	58·1	49·4	53·4	54·2
	Range	30–75	38–75	30–69	30–75	37–69
	S.D.	11·5	10·5	10·8	13·5	9·1
	Difference between means	—	(+)	(−)	(−)	(+)
			8·7, $P < 0{\cdot}02$		0·8, N.S.	
Pattern Perception	Mean	54·0	52·0	56·0	53·6	54·4
	Range	41–64	41–61	43–64	43–60	41–64
	S.D.	5·8	5·6	5·2	4·3	6·9
	Difference between means	—	(−)	(+)	(−)	(+)
			4·0, $P < 0{\cdot}05$		0·8, N.S.	

Table 2. *Intertest correlations*

Test	Group	n	Self-judging Vocabulary 10	Pattern Perception	Word-in-context. Number of sets attempted
Word-in-context	Total	40	0·39 ($P < 0{\cdot}02$)	−0·02	—
Total score	Arts,	20	0·04*	−0·21	—
	Science,	20	0·75* ($P < 0{\cdot}001$)	0·33	—
Mean per word	Total	40	0·44 ($P < 0{\cdot}01$)	0·11	0·18
	Arts,	20	0·20	−0·01	−0·02
	Science,	20	0·71 ($P < 0{\cdot}001$)	0·33	0·40
Number of sets attempted	Total,	40	0·13	−0·16	—
	Arts,	20	−0·21	−0·24	—
	Science,	20	0·22	0·08	—
Pattern Perception	Total,	40	0·18	—	—
	Arts,	20	0·45 ($P < 0{\cdot}05$)	—	—
	Science,	20	0·27	—	—

* Difference between pair of correlations significant ($P < 0{\cdot}05$).

demanded of and practised by, the scientist or mathematician rather than the arts student. The present evidence suggests that, if the Word-in-context is testing anything, it is not what is tested by classical high-grade tests of intelligence.*

Let us now consider the intercorrelations between the three tests, first for the total sample ($n = 40$) and then for the subgroups ($n = 20$). The correlations for the total sample between the Word-in-context and (*a*) the 10 Vocabulary, (*b*) the Pattern Perception tests are, respectively, 0·39 and −0·02. The former correlation is significant at the 0·02 level. It is to be expected that students with stronger vocabularies should tend to do better than those with weaker vocabularies on any meaningful, verbal test. The association is, however, low enough to suggest that the Word-in-context is not simply 'another vocabulary test'. Further reference will be made to this point below, when arts and science Word-in-context scores, separately, are compared with the Vocabulary test results.

More surprising is the lack of relationship ($r = -0{\cdot}02$) between Word-in-context and Pattern Perception. A low correlation would be understandable, since the Word-in-context is wholly verbal and Pattern Perception is wholly diagrammatic. But an r of less than 0·1 in either direction suggests that the type of reasoning in the two tests is interestingly unrelated. It looks again as though whatever the Word-in-context may be assessing, it is largely other than what the traditional high-grade intelligence test assesses.

Intertest correlations have been computed also for the subgroups, men/women and arts/science. Since the men/women correlations were all very similar, they are not included in Table 2. There are, however, some interesting differences between the arts and science intercorrelations. The most striking one is that between Word-in-context and 10 Vocabulary, the arts correlation being 0·04 and the science correlation being 0·75. The latter is significant at the 0·001 level and, despite the paucity of the numbers, the difference between the two correlations is also significant.

The Word-in-context/Pattern Perception intercorrelations for arts and science are less dramatic but again there are differences and again it is the scientists who show the closer association. In fact, the arts subjects show a negative (though not significant) relationship between Word-in-context and Pattern Perception score. The only case in which the arts people yield a closer association than the scientists is in the Pattern Perception/Vocabulary comparison.

The mean number of words in which the subject expressed his final verdict was calculated. Length of final verdict was, to the experimenters' surprise, found to be unrelated to arts/science; and also to goodness of score on the Word-in-context test.

Two further series of comparisons were made: (*a*) between the test scores of men and women, (*b*) between test scores and university examination results. The findings for both (*a*) and (*b*) were inconclusive. None of the differences in score between men and women approached significance. On each of the three tests, the mean scores differed by less than 2 marks (see Table 1), the women scoring the more highly. Little difference was found also between the scatter of scores gained by the two sexes, respectively.

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* Since this paper was written, Word-in-context scores (mean-per-word) have been correlated with AH 6 scores for 89 Oxford Arts scholarship candidates, yielding an r of $+0{\cdot}33$ ($P < 0{\cdot}01$). For the same group the uncorrected split-half (odd/even) reliability was $+0{\cdot}503$.

A comparison between the test scores and class obtained in university examination (the latest examination taken) proved equally inconclusive.* There was some slight evidence (found also in previous work) of agreement between 10 Vocabulary and examination class, but no association was found between either of the other tests and examination class. This may be due to the inappropriateness of the tests or to the paucity of the numbers. It is just possible, too, that the students tested did not form a representative group since the men/women results were a little unusual, most previous experiments having shown men to gain higher rather than lower scores than women, at this level, and also to yield generally a wider scatter (Fitt & Rogers, 1950; Watts, 1953).

VI. DISCUSSION

The experimental results suggest that the Word-in-context may be a practicable means of differentiation among subjects at a high level and that the basis of differentiation is probably new. It is perhaps worth noting that many subjects went out of their way to express their enjoyment in doing the Word-in-context and their interest in the outcome. This is by no means the rule among subjects for psychological experiments, psychometric or otherwise, and it seems to indicate that the test may well deal successfully with some of the motivational problems of testing highly intelligent and critical young adults.

The main points of interest concern the Word-in-context differences between arts and science and, in particular, those regarding (*a*) speed and power, (*b*) Vocabulary scores, and (*c*) Pattern Perception. We shall also discuss briefly the relatively high scores gained by the arts subjects on the Word-in-context test.

It is permissible to take the *number of sets attempted* as an index of speed and the *mean-per-word* as an index of power, in the Word-in-context, since the former is entirely unconcerned with accuracy and the latter has absolutely no relation to rate of working. It may be seen in Table 2 that whereas for the arts people the correlation between these two indices is $-0{\cdot}02$, for the scientists the correlation is 0·40. This is a marked difference. It suggests a closer association between speed and accuracy among the science than the arts students. This difference is far greater than the difference between the mean-per-word of the arts people and the scientists (0·12, see Table I); and it is not due to differences of scatter since the scientists have, on Word-in-context, a *smaller* standard deviation than have the arts students.

The scientists show closer associations (see Table 2) between Word-in-context on the one hand and both Vocabulary and Pattern Perception on the other, than do the arts subjects. The difference between the two subgroups on Vocabulary/Word-in-context total score is statistically significant at the 0·05 level. All these results seem to suggest that the scientists are more self-consistent, more 'of a piece' than the arts people. On the science (and mathematics) side, people who do well (or poorly) in one respect tend to do well (or poorly) in other respects. The arts students are evidently more 'random'—whether we are considering two cognitive qualities or speed/power.

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* Since this paper was written, some further validating data on the Word-in-context have been obtained, with younger, less highly selected subjects. A correlation of $r = 0{\cdot}479$ was found between the test and English Language G.C.E. 'O' level results, for 135 schoolchildren.

It will be interesting to see if this result is maintained on larger numbers. The only exception on the present data are the 10 Vocabulary/Pattern Perception correlations, which are higher for the arts than the science group, but not significantly so. It is interesting that this association should be higher than that between either of the other two tests, for this sub-group, since Vocabulary/Pattern Perception do not have in common either reasoning or words.

The testing in this experiment was carried out by J. M. Cook as part of her research project for the Part II Psychology Tripos. The other two authors wish to express their thanks to the Medical Research Council for financial support.

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Appendix I

Contexts for GODOWN

110P

On the way to the Quai Mytho I passed several ambulances driving out of Cholon heading for the Place Garnier. One could almost reckon the pace of rumour from the expression of the faces in the street, which at first turned on someone like myself coming from the direction of the Place with looks of expectancy and speculation. By the time I entered Cholon I had outstripped the news: life was busy, normal, uninterrupted: nobody knew.

I found Mr Chou's godown and mounted to Mr Chou's house. Nothing had changed since my last visit. The cat and dog moved from floor to cardboard box to suitcase like a couple of chess knights who cannot get to grips. The baby crawled on the floor, and the two old men were still playing Mah-jongg. Only the young people were absent. As soon as I appeared in the doorway one of the women began to pour out tea. The old lady sat on the bed and looked at her feet.

Graham Greene: *The Quiet American*, p. 224. Heinemann, 1955

110Q

Of course, this thought was not clear in my mind at that time. Then and for years after, my idea of fate—for in fate I firmly believed—was of a vague, floating, stupendous power, for which I felt only resentful wonder.

Another puzzle came one midsummer airing day. It seems odd that it should have happened then, for airing days were the most carefree, happy time of the year for me. Then the godowns were emptied and long ropes stretched in the sunshine, on which were hung torn banners bearing our crest, old field-curtains used in the camps of our ancestors, ancient regalia of house officers, and many odd-shaped garments belonging to what Ishi's fairy tales called 'the olden, olden time'. Beneath the low eaves were piles of clumsy horse armour bound with faded ropes of twisted silk; and old war weapons—spears, battle-axes, bows, and sheaves of arrows—stood in out-of-the-way corners of the garden. All available space was utilized; even the bridge-posts and the stone lanterns were decorated with chain-silk armour and lacquer helmets with fearful masks.

Etsu Inagaki Sugimoto: *A Daughter of the Samurai*, p. 138. Hurst and Blackett, 1933.

110R

We berthed towards sunrise in a gloomy and featureless roadstead, before a town whose desolate silhouette suggested that of a tin-mining village in the Andes. An unlovely straggle of godowns, patched and pelling, fronted the shallow and dirty shore. Here and there along the flat coastline with its unhealthy suggestion of salt-pans (I was not wrong: Limassa lies upon a shallow lake), here and there the eye picked out a villa of some style or consequence in a flowering garden. But even at this early hour the sunlight created a dense haze while the humid air of the little port came out across the still sea to meet us.

Lawrence Durrell: *Bitter Lemons*, p. 22. Faber and Faber, 1957.

APPENDIX II

Scoring

Test-word: GODOWN (store or warehouse in the far East).

Final verdict score		
5	(i)	Store or warehouse
	(ii)	Some sort of store or warehouse—generally used of a rather ramshackle place
4	(i)	A shed or similar resting place for collected paraphernalia
	(ii)	Some sort of building. Perhaps a shed or storehouse
3	(i)	A building attached to a large dwelling, peculiar to Japan, serving the same function as an English junk-room
	(ii)	Old hut for living or storage
2	(i)	Barn or shack
	(ii)	A shed, out-house or hut
1	(i)	Beach hut (or Eastern equivalent)
	(ii)	Rubbish bins, sometimes found in public places obviously customary and in no way to be looked down on though not necessarily decorative—just to serve their purpose.
0	(i)	Apartment block
	(ii)	Slum doss-house
−1	(i)	American slang to mean street
	(ii)	Latrine
−2	(i)	Boat
	(ii)	A painted sort of cart for transporting people—an open vehicle.

Thus it may be seen that the answers scoring 5 have absolutely the right idea and only the right idea. 4 is still correct but slightly less definite and 3, again, is a degree less good.

Answers scoring 2 make no implicit mention of storing, or its equivalent, and 1's may not even imply this.

0 answers are positively mistaken but they still indicate some sort of building. −1 answers are not even buildings and −2's are not even stationary.

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INDIVIDUAL DIFFERENCES IN RATE OF ACQUISITION, NEGATIVE TRANSFER, AND RATE OF RECOVERY IN DIGIT-SYMBOL SUBSTITUTION

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Male and female subjects worked at a digit-symbol substitution test under conditions of massed practice for six trials (acquisition phase). The order of symbols was then completely reversed to effect sudden disruption in performance (negative transfer). Subjects then worked at the test with reversed key for six trials under conditions of massed practice (recovery phase). Predictions about rate of acquisition, degree of disruption, and rate of recovery were based on multiplicative-drive theory and on Eysenck's theory of extraversion/introversion and the inhibition/excitation balance.

Results show that degree of disruption is positively correlated with both rate of acquisition and rate of recovery for males and females. Differences in MPI neuroticism and extraversion scores are not associated with the predicted differences in performance at the task. In discussion it is suggested that attempts to incorporate personality variables into learning theory need to consider different ways in which situations can differ, and other conceptions of the learning process in addition to multiplicative-drive theory.

I. Introduction

In his 1957 Presidential address to the American Psychological Association, Cronbach (1957) argues persuasively for the need to bring about a convergence of 'the two disciplines of scientific psychology', viz. experimental psychology and the psychology of individual differences. That there is a trend towards such a convergence is evident in the recent attempts to relate differences in learning and performance to personality variables such as manifest anxiety (Taylor, 1951; Taylor & Spence, 1952), test anxiety (Mandler & Sarason, 1952), achievement-related motivation (Lowell, 1952; Atkinson, 1960), and extraversion-introversion (Eysenck, 1957, 1960). The present investigation falls within this general area in its attempt to explore individual differences in acquisition, negative transfer, and recovery using concepts from learning and personality theory.

In the situation investigated, a subject works at a digit-symbol substitution test under conditions of massed practice for a short number of trials. This is called the *acquisition phase*. The order of symbols is then completely reversed so that the symbols previously substituted for digits 0, 1, ..., 9 are now substituted for digits 9, 8, ..., 0. Such a reversal should produce *negative transfer* (Osgood, 1953; Deese, 1958) as indicated by a sudden disruption in performance. The subject is now required to work at the test with reversed symbols under conditions of massed practice for the same number of trials as previously. This is called the *recovery phase*. The investigation has two main aims: (1) to examine the interrelationships between measures of rate of acquisition, degree of disruption, and rate of recovery, (2) to investigate the relationship of these measures to Eysenck's personality dimensions, viz. extraversion-introversion and neuroticism, as measured by the Maudsley Personality Inventory (MPI).

Using Hull's multiplicative-drive theory (Hull, 1943) we predict *positive* intercorrelations between measures of rate of acquisition, degree of disruption, and rate of recovery.* The multiplicative-drive theory leads to different predictions about the relationship of performance to differences in drive level (D) depending on whether the task is simple (involving few competing tendencies) or complex (involving strong competing tendencies). Where the task is simple, subjects with high D are expected to perform more efficiently than subjects with low D, but, where the task is complex, subjects with low D are expected to show superior performance. Generally speaking these predictions are supported in a number of studies initiated by the Iowa school (Taylor, 1951; Spence & Taylor, 1951; Taylor & Spence, 1952), which use the Taylor Manifest Anxiety Scale (MAS) (Taylor, 1953) to define differences in drive level. These studies have been reviewed by Taylor (1956), Jones (1960), and Brown (1961). Eysenck (1957) and Jones (1960) resurrect the Yerkes–Dodson law (Yerkes & Dodson, 1908) which asserts a curvilinear relationship between efficiency of performance and drive strength, and an optimal drive strength for efficient performance which varies inversely with the difficulty of the learning task.

We consider the digit-symbol substitution task used in the present investigation to be simple rather than complex. The correct answers to the digits do not have to be discovered but are prescribed by the key, and competing responses appear to be minimal. It follows that subjects who are high in D should tend to learn more rapidly in the acquisition phase than subjects who are low in D. When the symbols are completely reversed, however, the learned responses to the digits are no longer correct but they are likely to be at or near the top of the available hierarchy of responses and thus likely to dominate. These learned responses must be replaced by new responses which are lower in the hierarchy. Thus, with reversal of the symbols, the situation becomes one which involves strong competing tendencies and, according to multiplicative-drive theory, disruption in performance following reversal should be greater for subjects with high D than for subjects low in D. During the recovery phase a subject not only has to learn the new responses to the digits but he also has to overcome interference from the previously learned responses which are elicited by the digits and which compete with the new responses. Hence the recovery phase is relatively more complicated than the acquisition phase. Even so, the task is still essentially simple, since the correct associations are available in the changed key which can be referred to by the subject whenever he desires. Therefore, we also expect subjects with high D to learn more quickly in the recovery phase than subjects with low D. However, because the recovery phase is relatively more complex than the acquisition phase, we are less confident in predicting a positive relationship between rate of learning and strength of D for the recovery phase. It follows from the above considerations that measures of rate of acquisition, degree of disruption, and rate of recovery should tend to be *positively* intercorrelated.

It is further predicted that rate of acquisition, degree of disruption, and rate of recovery should be higher among subjects high in neuroticism than among subjects low in neuroticism, where neuroticism is measured by the Maudsley Personality Inventory (MPI). A neuroticism scale, the Maudsley Medical Questionnaire (MMQ), has been shown to correlate 0·92 with the Taylor MAS (Franks, 1956). Bendig (1957), using a sample of 254 undergraduate students, reports a correlation of 0·77 between scores on Taylor MAS and MPI neuroticism scores.* Eysenck (1957) and Jones (1960) consider neuroticism in terms of drive strength, and Lynn (1959) has made this assumption in investigating whether good academic achievers tend to be relatively high in neuroticism. If we assume that differences in neuroticism scores from the MPI reflect differences in level of D, the above prediction follows directly from the argument presented in the preceding paragraph.

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* I am indebted to R. P. McDonald who suggested this prediction and the theoretical basis for it.

Finally, we predict that rate of acquisition and rate of recovery should be higher for subjects low in extraversion than for subjects high in extraversion, where extraversion is measured by the MPI. Eysenck (1957) postulates that extraverts are individuals in whom excitatory potential is generated slowly to a weak level, and in whom reactive inhibition develops quickly and dissipates slowly. In contrast, introverts are assumed to be individuals in whom excitatory potential is generated quickly to a high level, and in whom reactive inhibition develops slowly and dissipates quickly. It follows that extraverts should condition less readily than introverts, and studies by Franks (1956, 1957, 1960), using eyeblink conditioning, support this prediction. In fact, Franks (1957) reports a correlation coefficient of −0·46 between extraversion and conditioning, although other studies (cf. Willett, 1960) have not yielded relationships of this order. Eysenck's assumptions, as applied to the present situation, suggest that introverts should tend to show more rapid learning in the acquisition and recovery phases of the digit-symbol substitution test than would extraverts. Furthermore, since reactive inhibition is assumed to build up with massed practice we would expect differences between introverts and extraverts to become more apparent in later trials. Therefore, differences in speed of learning between introverts and extraverts should be more obvious in the recovery phase than in the earlier acquisition phase.

II. PROCEDURE

A digit-symbol substitution test was designed with the following properties. The key consisted of digits from 0 to 9 with a different symbol associated with each digit. Capital letters were used as the symbols. Two keys were employed such that the order of symbols in key 1 was reversed in key 2. Two forms of the digit-symbol test were constructed to allow for counterbalancing of keys. In form A, key 1 preceded key 2; in form B, key 2 preceded key 1. In each form one key was used for six trials and then the reversed key was used for the remaining six trials. A set of 72 randomly ordered digits was presented on each trial, arranged in three rows of 24 digits. Six sets of these 72 digits were constructed for each key and these sets were randomly permuted over the six trials with a particular key.

Subjects were 36 male and 46 female undergraduate students from an introductory psychology class at the University of New England. Scores on the MPI were available for these subjects. The digit-symbol substitution test was administered under group conditions such that half the subjects did form A of the test and the other half did form B. Instructions were as follows: 'This is a test to see how well you can learn relationships. It has had extensive use in intelligence scales such as the Wechsler–Bellevue. At the top of each page is a key with numbers from 0 to 9. Associated with each number is a letter. Below the key are three rows of boxes with the numbers but not the letters. Your task is to write the appropriate letters in the empty boxes, starting with the first row of boxes and working from left to right along the row. You will have 40 sec. for each trial. I shall tell you when to start, stop and turn over. Try to work as quickly and as accurately as you can. Remember that this is a test of learning ability so try to do your very best.'

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* A study currently in progress at the University of New England, using a sample of 272 Teachers' College students, indicates a correlation of 0·67 between Taylor MAS scores and MPI neuroticism scores (Personal communication from James McCawley.)

Conditions of task administration were such as to emphasize rapid performance. There were no rest pauses between trials. At the experimenter's command subjects stopped work at each trial, turned the page, and immediately began working at the next trial. The small amount of time (40 sec.) allowed for each trial, and the 'test' orientation given to subjects, would tend to discourage subjects from checking back over their performance for accuracy. In fact, the highly speeded nature of task performance is evident from the fact that, on the average, subjects coded each digit in less than two seconds. At the beginning of trial 7 the experimenter indicated to subjects that the key had changed, and they began trial 7 immediately thereafter.

Each subject worked at the test for twelve trials under these conditions of massed practice. Trials 1 to 6 constituted the acquisition phase; trials 7 to 12, in which the original key was reversed, constituted the recovery phase. Analyses of variance indicated that mean total performance scores for form A and form B of the digit-symbol substitution test did not differ significantly for either males or females. Hence data obtained from both form A and form B were pooled for male and female groups. Analyses of results were performed separately for the combined male group and for the combined female group.

To perform these analyses, both male and female groups were split into equal thirds,* first on the basis of their distributions of extraversion scores, and then on the basis of their distributions of neuroticism scores. It was then possible to combine an analysis of differences in performance between subjects classified as high, medium, or low on each personality dimension, with an analysis of performance differences between trials, between the acquisition phase and the recovery phase, and for the different interactions. These data were analysed according to the split-split plot procedure presented by Collier (1958, pp. 230–5).

III. RESULTS

Table 1 contains the mean number of symbols substituted on each trial for male subjects classified as high, medium, or low in neuroticism, and for male subjects classified as high, medium, or low in extraversion. Table 2 presents corresponding information for female subjects.

The analyses of variance indicate that mean differences in performance between trials are highly significant (for males, $F = 67{\cdot}13$, DF = 5 and 330, $P < 0{\cdot}001$; for females, $F = 96{\cdot}44$, D.F. = 5 and 420, $P < 0{\cdot}001$). That learning did occur in both the acquisition phase (trials 1 to 6) and the recovery phase (trials 7 to 12) is apparent in Tables 1 and 2, and was certainly to be expected. Tables 1 and 2 also show the sudden disruption in performance that occurred from trials 6 to 7 when the key was reversed. The negative transfer effect is quite apparent. The analyses of variance further indicate that the interactive effect of trials and phase on performance is highly significant (for males, $F = 6{\cdot}00$, D.F. = 5 and 330, $P < 0{\cdot}001$; for females, $F = 4{\cdot}76$, D.F. = 5 and 420, $P < 0{\cdot}001$). This interactive effect can be seen in Tables 1 and 2 where the results indicate that mean performance scores do not rise to the same level in the recovery phase as in the acquisition phase, although improvement in performance does occur during both phases.

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* It was necessary to exclude randomly one female subject in order to obtain equal thirds.

Table 1. *Mean performance scores of males on digit-symbol substitution test in relation to neuroticism and extraversion scores* ($N = 36$)

	Neuroticism			Extraversion			All males	
Trial	High	Medium	Low	High	Medium	Low	Mean	S.D.
1	20·33	20·75	19·83	22·25	18·92	19·75	20·31	6·61
2	24·25	23·50	23·17	23·08	23·08	24·75	23·64	4·04
3	27·50	27·42	25·50	27·42	25·67	27·33	26·81	4·42
4	28·25	28·33	27·17	28·67	27·25	27·83	27·92	4·82
5	29·25	29·75	26·42	28·75	27·00	29·67	28·47	5·29
6	29·25	30·50	25·08	28·50	27·42	28·92	28·28	5·67
7	22·75	21·92	21·08	22·25	21·25	22·25	21·92	3·37
8	26·33	25·75	25·17	25·58	24·58	27·08	25·75	4·07
9	27·17	26·92	25·42	27·50	25·42	26·58	26·50	4·27
10	26·92	27·67	25·25	27·00	25·83	27·00	26·61	3·51
11	27·83	27·50	26·50	27·00	26·00	28·83	27·28	4·19
12	27·67	27·67	25·50	27·58	25·83	27·42	26·94	4·41

Table 2. *Mean performance scores of females on digit-symbol substitution test in relation to neuroticism and extraversion scores* ($N = 45$)

	Neuroticism			Extraversion			All females	
Trial	High	Medium	Low	High	Medium	Low	Mean	S.D.
1	23·60	23·73	22·33	23·00	23·87	22·80	23·22	4·33
2	26·80	25·87	24·53	25·27	25·80	26·13	25·73	4·13
3	29·73	29·40	28·73	28·93	29·13	29·80	29·29	4·54
4	31·00	29·00	27·87	29·27	28·73	29·87	29·29	4·65
5	30·73	30·00	29·80	30·47	30·07	30·00	30·18	5·04
6	29·73	28·73	29·53	29·87	29·13	29·00	29·33	4·78
7	24·20	23·67	20·80	22·00	23·00	23·67	22·89	4·13
8	28·87	26·40	24·93	26·80	26·40	27·00	26·73	4·61
9	29·27	27·07	26·07	27·80	27·07	27·53	27·47	4·39
10	29·20	27·87	26·93	27·60	27·60	28·80	28·00	4·52
11	29·07	29·33	27·80	28·20	28·07	29·93	28·73	4·14
12	29·20	26·93	27·33	27·67	27·60	28·20	27·82	4·30

None of the main effects or interactions involving either extraversion or neuroticism is statistically significant. In particular, neither subjects classified as high, medium, or low in neuroticism nor subjects classified as high, medium, or low in extraversion, show significant differences in performance on the task. There are no significant interactive effects of neuroticism and trials on performance, or extraversion and trials on performance. In short, there is no evidence in the present study to suggest that introverts improve more rapidly at the digit-symbol substitution test than do extraverts, or that subjects high in neuroticism improve more rapidly over trials than do subjects low in neuroticism. Nor is there any systematic tendency for subjects high in neuroticism to suffer greater disruption in performance than subjects low in neuroticism when the key is changed on trial 7. These results are therefore not consistent with predictions.

Table 3 presents intercorrelations between measures of rate of acquisition, degree of disruption, and rate of recovery for male subjects. Table 4 presents corresponding information for female subjects. For each subject, the measure of rate of acquisition is the difference between performance on trial 1 and 6, the measure of degree of disruption is the difference between performance on trials 6 and 7, and the measure of rate of recovery is the difference between performance on trials 7 and 12.

Table 3. *Intercorrelations of measures of rate of acquisition, degree of disruption, and rate of recovery for male subjects* ($N = 36$)

	Degree of disruption	Rate of recovery
Rate of acquisition	0·53**	0·03
Degree of disruption	—	0·50*

** $P < 0{\cdot}001$ (one-tailed test). * $P < 0{\cdot}005$ (one-tailed test).

Table 4. *Intercorrelations of measures of rate of acquisition, degree of disruption, and rate of recovery for female subjects* ($N = 46$)

	Degree of disruption	Rate of recovery
Rate of acquisition	0·60**	0·20
Degree of disruption	—	0·54**

** $P < 0{\cdot}001$ (one-tailed test).

Tables 3 and 4 show that rate of acquisition and rate of recovery correlate positively with degree of disruption for both male and female subjects. These correlations are statistically significant and are consistent with prediction. However, the relationship between rate of acquisition and rate of recovery is not statistically significant for either males or females. This result is contrary to the prediction of a positive correlation between these measures. Thus, in the present study, those subjects whose performance scores rise to a relatively high level over the six trials of the acquisition phase tend to show larger performance decrements than do those subjects with a lower rate of acquisition, when the key is suddenly reversed on trial 7. Similarly, subjects who show high performance decrements from trials 6 to 7 tend to show more rapid recovery over the six trials of the recovery phase than do those subjects with relatively low performance decrements. But measures of rate of acquisition and rate of recovery do not show significant interrelationships in the present investigation.

These trends are also apparent in Tables 5 and 6. Table 5 contains the mean number of symbols substituted on each trial for male subjects classified as high, medium, or low in rate of acquisition (trial 6–trial 1), in degree of disruption (trial 6–trial 7), and in rate of recovery (trial 12–trial 7). Table 6 presents corresponding information for female subjects. The high, medium, and low categories are defined in terms of top, middle and bottom thirds of the respective distribution of scores.

Table 5. *Mean performance scores of males on digit-symbol substitution test in relation to rate of acquisition, degree of disruption, and rate of recovery* ($N = 36$)

	Rate of acquisition			Degree of disruption			Rate of recovery		
Trial	High	Medium	Low	High	Medium	Low	High	Medium	Low
1	15·17	20·92	24·83	20·92	21·25	18·75	20·67	20·92	19·33
2	23·00	23·42	24·50	24·67	23·75	22·50	24·58	23·58	22·75
3	26·75	26·42	27·25	29·08	26·67	24·67	28·83	26·58	25·00
4	28·42	27·58	27·75	31·17	27·58	25·00	30·00	28·00	25·75
5	29·42	28·25	27·75	32·50	28·42	24·50	30·58	28·33	26·50
6	30·33	27·75	26·75	32·83	28·33	23·67	29·42	28·33	27·08
7	20·75	22·42	22·58	20·75	22·67	22·33	20·08	22·67	23·00
8	24·50	25·92	26·83	25·42	26·17	25·67	26·17	26·17	24·92
9	25·92	26·58	27·00	27·17	26·92	25·42	27·33	26·33	25·83
10	25·58	27·42	26·83	27·58	26·67	25·58	27·25	27·00	25·58
11	26·67	27·92	27·25	28·92	26·83	26·08	28·33	27·33	26·17
12	26·25	27·33	27·25	28·25	26·42	26·17	29·33	27·33	24·17

Table 6. *Mean performance scores of females on digit-symbol substitution test in relation to rate of acquisition, degree of disruption, and rate of recovery* ($N = 45$)

	Rate of acquisition			Degree of disruption			Rate of recovery		
Trial	High	Medium	Low	High	Medium	Low	High	Medium	Low
1	21·53	22·33	25·80	23·13	22·93	23·60	23·33	23·60	22·73
2	25·13	25·40	26·67	25·67	26·47	25·07	26·47	25·40	25·33
3	29·73	28·80	29·33	29·80	29·80	28·27	30·87	27·73	29·27
4	29·93	28·00	29·93	29·73	29·67	28·47	30·80	28·00	29·07
5	31·53	29·67	29·33	32·13	30·20	28·20	32·40	28·67	29·47
6	32·27	28·80	26·93	31·87	30·40	25·73	31·67	28·20	28·13
7	22·20	23·13	23·33	20·33	24·53	23·80	20·93	23·13	24·60
8	27·00	25·80	27·40	25·73	28·20	26·27	26·60	26·07	27·53
9	27·87	26·60	27·93	26·67	28·53	27·20	27·73	27·27	27·40
10	28·60	27·00	28·40	27·33	29·73	26·93	29·13	27·73	27·13
11	29·40	27·80	29·00	29·27	28·73	28·20	30·40	27·73	28·07
12	28·27	27·47	27·73	27·60	29·13	26·73	30·20	27·40	25·87

It is apparent from Tables 5 and 6 that subjects classified as high in rate of acquisition show a greater average drop in performance from trial 6 to trial 7 than do subjects classified as medium or low in rate of acquisition. Similarly, subjects classified as high in rate of recovery show a greater average drop in performance from trial 6 to trial 7 than do subjects classified as medium or low in rate of recovery. But subjects classified as high, medium, or low in rate of acquisition are not necessarily high, medium, or low respectively in average rate of recovery. Similarly, subjects classified as high, medium, or low in rate of recovery are not necessarily high, medium, or low respectively in average rate of acquisition.

Finally, it is interesting to note that, in the population used, neuroticism and extraversion scores are negatively correlated for females ($r = -0{\cdot}46$, D.F. = 44, $P < 0{\cdot}01$), but are not related for males ($r = 0{\cdot}05$, D.F. = 34, N.S.). Mean neuroticism scores are as follows: males, $M = 23{\cdot}86$, S.D. = 10·79; females, $M = 25{\cdot}39$, S.D. = 11·15. Mean extraversion scores are as follows: males, $M = 27{\cdot}40$, S.D. = 8·64; females, $M = 24{\cdot}78$, S.D. = 8·56. Differences between males and females in neuroticism and extraversion scores are not statistically significant.

IV. DISCUSSION

The present investigation provides clear evidence of learning in both the acquisition and recovery phases of the digit-symbol substitution test, and disruption in performance (negative transfer) with the reversal in key on trial 7 (Tables 1 and 2). This negative transfer effect is consistent with the results of earlier studies (e.g. Bruce, 1933). The results also indicate that performance scores do not rise to the same level in the recovery phase as in the acquisition phase. This difference in performance between phases is understandable, since the recovery phase involves interference by competing responses established in the acquisition phase, and is more likely to be influenced by cumulative fatigue following massed practice.

Results do not support predictions from multiplicative-drive theory using the MPI neuroticism scores to reflect differences in drive level (D), nor do they support predictions from Eysenck's theory of extraversion-introversion using MPI extraversion scores. In fact, there is no indication in the present study that performance scores bear any systematic relationship to neuroticism or extraversion. There are at least four possible reasons for these negative results.

(1) It might be argued that the digit-symbol substitution test is a complex rather than a simple task, at a much higher level of difficulty than the conditioning procedure used by Taylor (1951) and Franks (1956). However, on analysis, the digit-symbol task appears simple; the key is always present, and learning seems to involve an obvious association of symbols with digits, and eye-movements with digits. The recovery phase is more complex than the acquisition phase since interfering responses are elicited following the change in key. But the acquisition phase does not involve these complications, and there is no evidence of a positive relationship between rate of learning and neuroticism. If the recovery phase were to be labelled as complex we would expect subjects high in neuroticism to be less efficient learners than subjects low in neuroticism, but there is no evidence of such a difference in the present study. One might also argue that a subject could approach a digit-symbol substitution task with either a 'speed' set or an 'accuracy' set, and that, unless one knew which set was dominant, predictions about the relationship of performance to drive level would be ambiguous since high drive might facilitate speed, accuracy, or both. The writer believes, however, that the procedure of the present investigation established a dominant set among subjects to work as quickly as possible. If this set is assumed, the prediction of a positive relationship between rate of learning and neuroticism following from multiplicative-drive theory would appear reasonable, particularly for the acquisition phase.

(2) It might be argued that the length of practice on the digit-symbol substitution task was not sufficient to determine systematic differences in performance between

extraverts and introverts. More trials at the task would increase differences between extraverts and introverts in the reactive inhibition developed, and this would be reflected in performance differences. As against this argument, the present procedure involved massed practice with little opportunity for rest pauses. Hence one would expect the development of reactive inhibition. In fact, as we have indicated, there is a generally depressed performance by subjects in the recovery phase compared with the acquisition phase, and this interactive effect of trials and phase on performance is highly significant. This depressed performance in later trials is consistent with the assumption of a build-up in reactive inhibition, although it is probably also a function of interfering responses occurring during the recovery phase. There is no evidence of systematic differences in rate of learning between introverts and extraverts for these later trials, however, despite the implication that reactive inhibition has developed.

(3) It is possible that the MPI does not provide a good criterion for distinguishing between subjects high and low in neuroticism, or high and low in extraversion. Willett (1960, p. 192) suggests that, '...the E scale of the Maudsley Personality Inventory is a poor criterion in differentiating groups of introverts from groups of extraverts', and Eysenck (1960) also regards the MPI as severely limited as a criterion. However, both Eysenck (1957) and Jones (1960) relate differences in neuroticism to differences in drive strength, and both MPI neuroticism scores and MMQ neuroticism scores correlate highly with scores on the Taylor MAS (Bendig, 1957; Franks, 1957), which has been used extensively as a criterion for differences in D in learning experiments. Furthermore, Franks (1956), using the MPI extraversion scale as criterion, found that normal introverts condition more readily than normal extraverts. One might ask why the extraversion scale of the MPI predicts differences in performance in some situations but not in others.

(4) The preceding comments suggest that the theoretical models used in the derivation of hypotheses of the present investigation may be limited to the extent that they do not adequately take account of differences in situation. Psychological theories need to consider the important ways in which situations can differ, and this implies systematic analysis of the major dimensions along which situations can vary. This task is probably just as important as the specification of differences between individuals. We need a taxonomy applied to situations as well as a taxonomy applied to persons. Studies based on multiplicative-drive theory do consider differences in the complexity of the task. Even here, the concept of complexity seems to be limited to the degree to which competing responses can interfere with performance. Differences in situational complexity probably exist which are difficult to conceptualize in terms of competing responses. Rather they appear to depend more upon organization and structure. Furthermore, it is likely that there are other differences between situations which are important when one is analysing the relationship of learning and performance to personality variables. The following three characteristics of a situation appear to the writer to be worthy of further investigation:

(*a*) The *degree of uncertainty* of a situation. By this we mean the degree to which aspects of a situation are ambiguous and unpredictable. It may be argued that motivational effects become more apparent as a situation becomes more uncertain. This assumption is, of course, a basic one in projective testing, but there is also evidence for it in experimental studies, e.g. Sutcliffe's (1955) investigation* of task variability and the level of aspiration (cf. Feather, 1963). In the present study, the digit-symbol substitution test is low in 'uncertainty'. Scores tend to increase fairly steadily from trial to trial, and a subject can predict improvement with practice. Thus, in line with the preceding argument, the situation is not one in which we would expect to find obvious relationships between performance and personality dimensions.

(*b*) The *degree of stress* induced by the situation. By this we mean the degree to which the situation elicits stress in the person. Jones (1960, p. 492) remarks that '...it may be that the high drive of neurotics is aroused only in situations of threat or "ego-involvement".' Sarason, Mandler & Craighill (1952), using the Test Anxiety Questionnaire (TAQ), found that high anxious subjects were inferior in performance to low anxious subjects on a digit-symbol substitution task in a stress condition where subjects were told that they were expected to finish the task but where, in fact, this could not happen. Westrope (1953), using a digit-symbol substitution task, found that performance declined with a shift from control to stress conditions. There was a tendency (not significant) for the decrement in performance to be greater for high anxious subjects than for low anxious subjects, where differences in anxiety were defined by the Taylor MAS. Although the instructions employed in the present study were designed to be ego-involving, no explicit attempt was made to induce stress. In fact, the low degree of 'uncertainty' in the task probably militated against obtaining wide variations among subjects in degree of anxiety elicited.

(*c*) The *judged difficulty* of the situation. By this we mean the degree to which a situation is judged by a subject as easy or difficult. Sarason (1961), using a very difficult anagrams test, found that high anxious (TAQ) subjects obtained lower performance scores when told that they should be able to complete the task than when told that the task was difficult and hard to complete. Just the reverse was true of low anxious subjects. Feather (1961), basing predictions on a 'motive-expectancy-value' model, found that subjects in whom, by assumption, the motive to avoid failure (M_{af}) was stronger than the motive to achieve success (M_s), persisted longer at a task when it was presented to them as very difficult than as easy. Subjects in whom it was assumed that $M_s > M_{af}$ did just the reverse. These two studies may be conceptualized in terms of the expectations of success which a situation elicits among subjects and the interaction of such expectations with relatively stable personality dispositions in determining behaviour (cf. Feather, 1962).

Complexity, degree of uncertainty, degree of stress, and judged difficulty are not presented as independent characteristics of a situation. Most likely they are interrelated. They do appeal to the writer as aspects of a situation which deserve further investigation in relation to learning and performance.

The results of the present investigation show that rate of acquisition is not significantly related to rate of recovery (Tables 3 and 4). Failure to obtain a significant correlation between these measures may reflect the greater complexity of the recovery phase where competing responses and fatigue interfere with efficient performance. The positive relationships between degree of disruption on trial 7 of the digit-symbol substitution test and both rate of acquisition and rate of recovery are consistent with predictions from multiplicative-drive theory. It should be noted, however, that these relationships are not necessarily due to individual differences in drive level (*D*). Even within drive theory these relationships could be predicted on the basis of individual differences in the ability to form associations or habits rather than in terms of differences in *D*. It is also possible that these relationships could be predicted from theoretical models outside of multiplicative-drive theory. For example, Ehrlich (1961) has shown that the person high in dogmatism (Rokeach, 1960) tends to learn more slowly in a classroom situation.* Perhaps such a person would also suffer relatively little disruption of performance under conditions of negative transfer, since his cognitive structures are 'closed' and not liable to interference. Future research on the role of personality variables in learning should not restrict itself to the multiplicative-drive model. Certainly the results of the present study do not provide unequivocal support for this model. It may pay dividends to approach this general area flexibly with both associative and cognitive types of learning theory in mind, and with an awareness of some of the important dimensions along which situations can vary.

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* The effects of anxiety on performance may become apparent in tasks of high variability since task-relevant responses are not substantially reinforced and relatively more task-irrelevant responses should tend to be evoked (cf. Mandler & Sarason, 1952).

Finally, results show that, in the population tested, neuroticism and extraversion scores from the MPI are negatively correlated for females and uncorrelated for males. Mean neuroticism scores of 23·86 for males and 25·39 for females are above the mean score of 18·63 quoted by Jensen (1958) for normals, but consistent with the mean score of 26·78 which he quotes for students (see also Lynn, 1959). Eysenck (1962) has recently indicated that extraversion and neuroticism scales may not retain their independence when normal samples with high neuroticism scores are tested. The negative correlation obtained for female subjects in the present study can be taken as further evidence for this assertion.

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* A recent replication of this study by Christensen (1963), however, failed to establish the predicted negative relationship between classroom learning and dogmatism.

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ON THE MEASUREMENT OF THE LEVEL OF SKIN POTENTIAL

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The use of the measurement of skin potential level as an alternative to skin resistance is considered. Methods for the reduction of artifacts in potential measurement are described.

INTRODUCTION

It is necessary at the outset to distinguish the measure of level of skin potential which represents an ongoing state of the organism from skin potential response which is the momentary fluctuation of the voltage recordable from the skin as the result of a stimulus. The mechanisms underlying these two aspects of electrical skin activity are not well understood. It is, however, not unlikely that the physiological functions giving rise to them are different (Wilcott, 1962). It is thus possible that the optimal conditions for the measurement of potential level may not be those which are optimal for the measurement of responses. While some of the points to be discussed undoubtedly apply to both types of measurement it is the reliable measurement of skin potential level that is the primary concern of this paper.

Until recently few workers concerned themselves with skin potential measurement, because skin resistance recording presented less technical difficulties. However, during the past few years the advent of low drift, high gain, high input impedance d.c. amplifiers has made the recording of skin potential as easy as that of skin resistance. The main disadvantage of these amplifiers is their high cost.

The main parameters involved in the measurement of skin resistance are: current flow, electrode size, nature and concentration of electrolyte (Floyd & Keele, 1936*a*, 1937). If a.c. resistance measurement is used in an attempt to overcome polarization difficulties, the additional parameter of frequency must be introduced. Floyd & Keele (1936*a*) showed that in contrast to skin resistance skin potential showed no significant effect of electrode size over a range from fine capillary to 3 cm.^{2}. With a high input impedance amplifier no effective current flows through the subject, so that the only important parameters which are involved in skin potential measurement are apparently the nature and concentration of the electrolyte. It would seem therefore that in so far as fewer parameters of measurement are involved there is an immediate possibility of reduction of error variance in skin potential as compared with skin resistance measurement. This is not necessarily so however. If, to take an example, skin resistance is measured with an impressed current of 10 μA., the voltage appearing across a subject with a typical resistance of 100 kΩ is 1 V. In the relation of this voltage to the usual maximum value of about 50 mV. which is typically found in skin potential measurement, it may be seen that the magnitude of tolerable error in skin resistance may be larger than that in skin potential measurement. Furthermore, while eliminating the factors of electrode size and current magnitude in skin potential measurement, possible sources of error such as inequality of temperature at the electrodes introducing error voltages of up to 3 mV. make themselves felt when dealing with a range of potentials which probably is confined to about 50 mV. The main advantage of skin potential measurement to balance against the need for greater care with its measurement is that by removing the need for the passage of current through the skin the polarization of the skin between the electrodes is eliminated. The effect of polarization cannot readily be distinguished from the value of resistance which it is desired to measure (Floyd & Keele, 1936*b*). A further feature of skin potential as compared to skin resistance is that it may be measuring a different aspect of the organism. While it is well established that sweat gland activity is involved in the determination of skin resistance, it is suggested that it is not directly concerned in the determination of skin potential level (Wilcott, 1958, 1962). It has been suggested (Höber, 1947) that the potential exhibited by animal membranes is the algebraic sum of at least three separate potentials. However, as this suggestion is based on work on frog skin, which has a very different structure from human skin, further work is required to determine whether human skin behaves in a similar manner. Nothing in the studies reported here suggests that more than one component is responsible for the potentials observed. There has, however, been no specific attempt to determine how many mechanisms are involved.

While not forgetting that the determinants of skin potential are not well understood, the aim of this paper is the presentation of an empirical method of measurement which produces results which are consistent with the idea of skin potential as an autonomic variable, and are sufficiently free from experimental error to be reliably repeatable.

Those sources of error which could be recognized have largely been investigated. Some parts of the method of measurement have been adopted following other workers' recommendations; others have probably been chosen on the basis of prejudice. If a method has been found to give satisfactory results, it has not been altered. Experiments have only been carried out on those parts of the total measurement chain which have been thought or found to be weak. As far as possible the aim has been to keep the artifact introduced at any point to below 0·1 mV.

ELECTRODES

Deficiencies in electrodes, which are not evident in a.c. recording, become marked sources of error when measuring standing levels of direct current. For the measurement of skin potential levels it is important that the electrodes should have low levels of standing potential when measured in saline solution, and should show small noise variations and negligible drift over time.

Silver–silver chloride electrodes were used. The technique to be described usually produced electrodes having a potential between them of less than 100 μV. Drift and noise were also adequately low. Figure 1 shows a sectional diagram of an electrode.

Disks $\frac{5}{16}$ in. in diameter were cut from 0·020 in. thick 99·99 % pure silver sheet in a miniature press. Jeweller's silver is not satisfactory for this purpose. The disks were washed for 10 min. in 2 N hydrochloric acid to remove any trace of iron left by the cutting process. They were then washed in distilled water and laid on a filter-paper. A lead of 7/0·0048 in. tinned copper wire, P.V.C. insulated, was soldered to one face, great care being taken not to let the solder run to the edge of the disk. Excess flux was scraped off and the disks washed in methylated spirit to dissolve any remaining flux.

Rubber rings in which the electrodes were set were formed from electric wiring grommets cut in half. V-shaped grooves cut in the cut surface of the grommets enabled the lead wires to lie flat. The rings were washed in methylated spirit to remove grease. The silver disks were pushed half-way into the rubber rings, which were then heated to drive off any methylated spirit remaining. 'Araldite' resin was dropped into the back of the ring until a slightly convex surface was obtained.

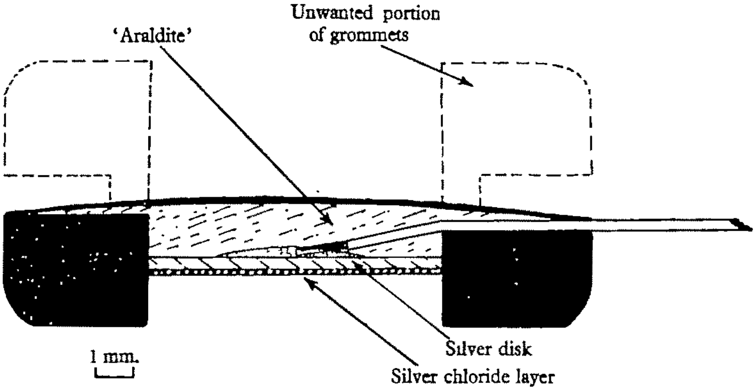

Fig. 1. Sectional diagram of electrode.

Heat was then applied until the resin set. In order that chloriding should be performed on a clean silver surface, electrodes were electrolytically etched by being made the anode in an electroplating cell consisting of a silver cathode and a silver nitrate solution. A current of 1 amp. was passed until it was seen that the top layer of silver was removed and a greyish-white slightly crystalline surface revealed. The electrodes were immediately washed in distilled water and placed in a 0·5 g./100 ml. potassium chloride solution. When a batch of six electrodes was etched they were chlorided by being made the anodes in a fresh 0·5 g./100 ml. KCl solution. Using a silver cathode, a current of 0·5 mA. per electrode was passed for 1 hr. The electrodes were then washed in distilled water and placed, with their leads shorted together, in a fresh 0·5 g./100 ml. KCl solution to age for a day. As in skin potential measurement no current is passed through the electrodes the depth of chloriding is not as critical as it would be if Ag/AgCl electrodes were prepared for skin resistance measurement. In this latter application chloride is stripped off by the passage of current.

Electrodes made in this way should not show a potential difference greater than 100 μV. on first testing immediately after being short circuited. If not rejected on this criterion they were then tested again after being opencircuited for 1 hr. A p.d. of greater than 100 μV. again caused rejection.

ELECTROLYTE

Potassium chloride was adopted as the electrolyte salt throughout the experiments to be reported following a recommendation by Rein (1929). This electrolyte has proved satisfactory for potential level measurement and sufficient work has now been done with it to make its continued use worth while. However, as Edelberg, Greiner & Birch (1960) have shown, it produced the smallest response of any of the electrolytes they examined and is thus not necessarily to be recommended in a study where the appearance of responses is important.

The determination of the most suitable concentration of electrolyte to use presented some difficulty. Where the electrode and electrolyte are merely conductors of some internally generated voltage, such as the electrical activity of the heart, a high concentration giving good conduction is the best to adopt. For instance, it can be shown that a 100-fold change of concentration of electrolyte produces no change in the amplitude of the E.C.G. but the noise level is lower at the higher concentration. However, when the electrolyte forms part of the voltage producer, as it does if the skin can be considered to act even approximately as a semi-permeable membrane, the voltage recorded is related to the electrolyte concentration.

To enable the most suitable electrolyte concentration to be determined the potentials resulting from its use must be related to some external criterion.

For other reasons skin potential as a measure of physiological 'arousal' was compared with another putative measure of arousal (Venables, 1963). It was found that the relationships between these measures was greatly affected by the electrolyte concentration. A low concentration, 0·1 g./100 ml. KCl, produced a proportion of subjects with positive skin potential whose data showed little relationship to the other arousal measure: too high a concentration, 10·0 g./100 ml. KCl, gave a piling up of the skin potentials of all subjects near an upper limit. To achieve an optimal distribution of potentials over a useful working range the best concentration was found to be 0·5 g./100 ml. KCl. With this concentration all potentials were negative, but extended over a range from about 2–50 mV. This concentration has now been adopted by us as a standard for skin potential measurement. The 0·5 g./100 ml. KCl was made up with 2·0 g./100 ml. agar-agar as an electrolyte jelly.

The electrolyte was made by dissolving the appropriate amount of agar-agar in the KCl solution heated until all the agar-agar dissolved. The mixture was then stirred mechanically all the time that cooling took place to avoid lumpiness. There are some indications that higher concentrations of agar-agar which result if lumps are allowed to form result in erroneously high skin potentials, presumably by the formation of an effectively additional membrane across which different potentials can appear. It has also been found that agar-agar electrolyte becomes stale after about a fortnight and more random and high-value skin potentials result. As an additional precaution it was found wise to keep the day's supply of electrolyte in a syringe, storing the main supply in an airtight jar. Relationship of skin potential to a second experimental variable was found to be 0·33, N = 20, when using stale electrolyte; whereas with fresh electrolyte and the same subjects the correlation rose to 0·79.

ELECTRODE PLACEMENT

The active electrode is traditionally placed on a palmar (or plantar) site. This choice of site has been followed in the present case; work on potentials from other sites, however, would be desirable. The more defined choice of what part of the palmar surface to use has to be made on several grounds. In considering the first, electrical, determinant it was found that the potential over the whole of the palmar surface proper differed by less than 2 mV, but the palmar surface of the fingers showed a mean drop of about 6 mV, compared with the remainder of the palm. The second determinant of site was mechanical. The traditional site in the centre of the palm is mechanically poor, because flexing of the hand causes the electrode to be lifted off the skin surface. The site finally adopted was the hypothenar eminence, a solid pad of flesh with little movement as the hand moves and little potential gradient over its surface. For a site in an experiment where the subject is expected to move fairly freely the skin over the proximal phalanx of one of the fingers is probably mechanically superior to the hypothenar eminence in that the adhesive tape attaching the electrode can be more securely fixed. Cleaning with ether was the only treatment given to the palmar site before affixing the active electrode.

The inner surface of the forearm was chosen as the site for the inactive reference earthed electrode. The ideal aimed at was a site which was electrically inactive, so that any potential measured would be referred to the electrical activity under the palmar electrode. Rupture of the stratum lucidum between the cornified and non-cornified epidermis allows ions from the electrolyte to pass freely in either direction with consequent lack of production of potential at the site (Rothman, 1954). To this end the continuity of the skin at the reference electrode site has to be broken. This can be done either by sandpapering, drilling (e.g. Shackel, 1959) or piercing. As the studies using skin potential which it was desired to carry out were mainly on psychiatric patients, piercing was considered prohibited, drilling undesirable and sandpapering the method of choice. Unfortunately the discomfort of the method is directly correlated with the objectivity with which its effectiveness can be seen.

Skin drilling and sandpapering were compared on ten normal subjects. Potential was measured on sites before and after drilling and sanding, and the state of the subject's potential meanwhile monitored by a second control electrode. Sandpapering was carried out by way of a $\frac{1}{4}$ in. strip of 'flour' paper fixed in a slot at the end of a Perspex rod. Sufficient rubbing was carried out to achieve slight erythema or in subjects with dry skin the appearance of a small amount of white powdery dust. Drilling was carried out with a dental diamond burr drill to achieve the appearance of a small shiny pit in the epidermis. No blood was drawn. It was found that the amount of change due to abrasion was related to the skin potential level given by unabraded skin. Those subjects with the lowest potential increased in potential most. There was, however, no significant difference either in slope or in intercept of the regression lines relating initial potential and the change due to sanding or drilling. The correlation between initial potential and change due to sanding was $-0{\cdot}86$, that between initial potential and change due to drilling $-0{\cdot}52$. The smaller correlation found with the drilling data was largely due to the greater variance in the changes in potential due to this method. This was shown most clearly if the standard errors of the regression lines relating change and initial potential were considered. For sanding $\sigma y.x = 2{\cdot}6$ mV. For drilling, $\sigma y.x = 6{\cdot}0$ mV. This was rather surprising in view of the more objective criterion of required amount of drilling.

Also measured in this experiment at the same time as the potential was its source resistance. This was done by putting sufficient resistance in parallel with the subject to reduce the measured potential by half. It was found invariably that the source resistance of unabraded skin was 1 MΩ or greater. Both sanding and drilling reduced this to about 100 kΩ. In order to determine, in a single subject, whether a value of source resistance achieved by a single drilled hole could be decreased by, so to speak, putting further holes in parallel, four more holes were drilled, and finally a hole deep enough to draw blood, but there was no change in source resistance. It would appear from these experiments that an adequate degree of sandpapering probably achieves the same results as rather heroic amounts of drilling. It is suggested that the most objective way of determining the correct amount of sanding is to use source resistance as the criterion. Probably 100 kΩ is an adequate figure. Sandpapering is obviously less disturbing than drilling and has therefore a distinct advantage in work with patients.

AMPLIFICATION AND RECORDING

The obvious requirements of this part of the total measurement chain are that the amplifier should have a gain which enables 0·1mV to be easily read and that its drift over the maximum time of the experiment should be well within this figure. Most important for skin potential measurement is the input impedance of the amplifier. If, for instance, it was attempted to measure the potential across unabraded skin having a source resistance of 1 MΩ with an amplifier whose input impedance was 1 MΩ, the amplifier would only measure half the actual voltage at the skin. With abraded skin the position would be clearly improved where with a source resistance of 100 kΩ 90 % of the actual voltage would be present across the amplifier. While it is possible to achieve input impedances of say 5 MΩ by standard techniques, higher values involve greater complexity. However, a high effective input impedance can be brought about by the use of a 'backing-off' circuit, which has the additional advantage of allowing the amplifier to be used at a higher gain, so that changes in potential can be measured as deviations from the backed-off potential. The backing-off circuit is a device which introduces a voltage in series with the subject and the amplifier of opposite polarity to the subject. In the practical version voltages of from 0–99 mV. in 1 mV. steps can be opposed to the skin potential, so that the resultant zero potential can appear at the centre of a recorder chart and skin potentials are measured as deviations on either side of it. At the point when the backing-off potential is equal to the skin potential no current flows and the effective input impedance of the amplifier is infinite. If, because the experimenter is watching some other part of the experiment, he allows the difference between the backing off and the subject's potential to reach as much as 5 mV., the effective input impedance will still be as high as 50 MΩ and the error in measurement will only be 0·2 % or 0·1 mV. in 50 mV.

A further advantage of the use of a backing-off circuit is that by short circuiting the subject's terminals the backing-off voltage appears across the amplifier and may be used for calibration purposes. Details of a backing-off circuit are shown in Fig. 2.

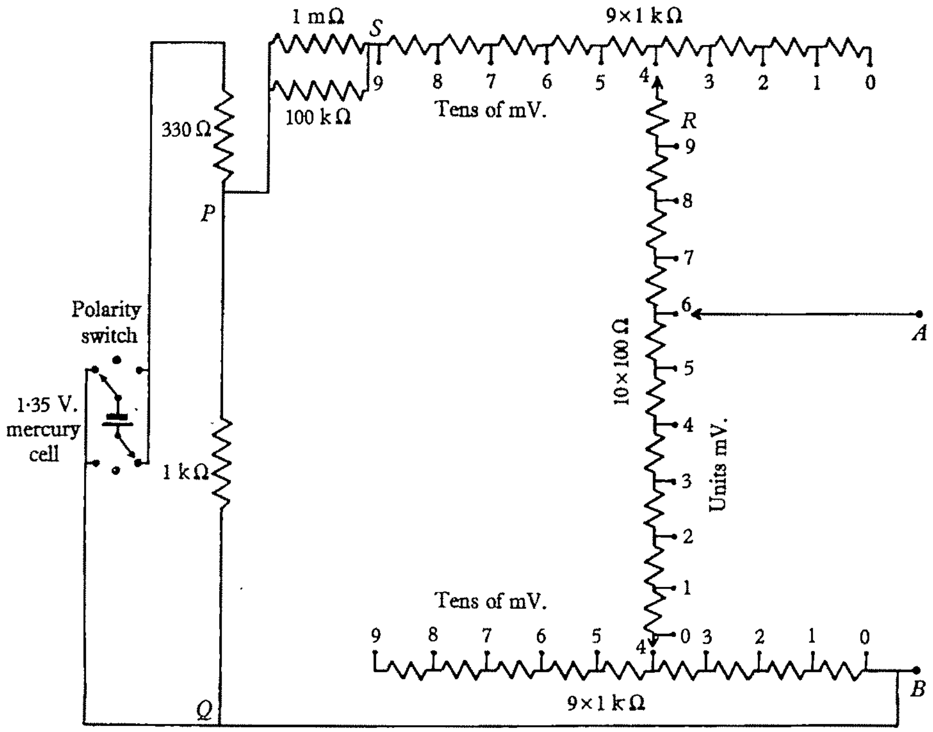

Fig. 2. Backing-off circuit. 1·35 V. from the mercury cell is divided, so that approximately 1 V. appears across points *P* and *Q*. A potentiometer chain of 1 MΩ in parallel with 100 kΩ, in series with the 10 kΩ derived from the 'tens' and 'units' chains ensures that 100 mV. appears across points *S* and *Q*. The upper and lower contacts of the two 9 × 1 kΩ 'tens' resistor chains moving together, and the 10 × 100 Ω 'units' chain ensure that a constant 10 kΩ is across points *S* and *Q*. With the top and bottom 'tens' contacts at 9 and the units contact at 9, 99 of the 100 mV. between *S* and *Q* appear at the terminals *A* and *B*. The extra 1 mV. is dropped across resistor *R*. Other voltages are obtained accordingly. In the diagram as an example the contacts are set at 46 mV.

THE EFFECT OF TEMPERATURE

Two temperature effects are possible in skin potential measurement. One is concerned with the effect of temperature on the production of voltage at the skin; the other is a potential difference resulting from having electrodes at different temperatures. If, as many have assumed, an approximation to a semi-permeable membrane is concerned in the production of skin potential, a maximum potential difference of a millivolt per 5° C. temperature difference over a range of skin temperatures of 20–35° C. is theoretically possible. If the whole limb warms evenly and there is no abrasion at the forearm site, there should be no effect of temperature, because the temperature will affect the potentials produced at each site equally. If, on the other hand, by the effective rupture of the membrane at the forearm site no voltage is produced there, temperature will have an effect, because it is affecting only the potential at the hand.

The relationship between hand temperature and skin potential over a group of subjects was not significant, but this is hardly surprising in view of the many more marked effects on skin potential due to individual differences. The more direct experiment of placing the whole arm of a subject in an enclosure whose temperature can be varied and comparing the potentials on this arm with those on the other control arm, whose skin temperature is also monitored, has not been performed.

The second effect of temperature is that produced by non-isothermal electrodes and is of the order of 450 μV per ° C. difference, the hotter electrode being positive.

In a study of the hand and forearm temperatures of sixteen normal and ten schizophrenic subjects it was found that the forearm temperature was remarkably constant with a mean figure of 30·5° C., range 29–31·5° C., while the hand temperature varied from 20·7 to 34·2° C. A potential of some 2 mV. due to this temperature difference was thus possible. By maintaining the room temperature between 19° and 21° C. the variation in hand temperature tended to be reduced after 3 or 4 min. but in order to eliminate completely this artifact the following technique was devised.

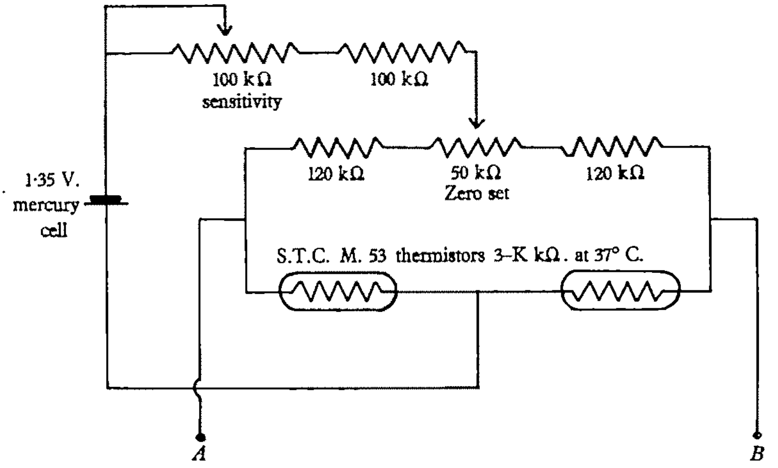

Fig. 3. Circuit for compensating error voltage produced by non-isothermal electrodes. The two thermistors are placed by the electrodes on the subject's hand and forearm. Differences in temperature cause differences in resistance of the thermistors, which in turn cause an out-of-balance voltage to appear at *A* and *B*. The voltage is adjusted by means of the sensitivity control to be 450 μV./ ° C.

Thermistors (resistors with high temperature coefficients) were placed alongside the hand and arm electrodes. These thermistors, which were mounted in half grommets similar to the electrodes, form two arms of a Wheatstone bridge. Differences in temperature at hand and arm cause an out-of-balance voltage to appear across the bridge. This voltage, which may be adjusted to be 450 μV. per ° C., is inserted in series with the subject, backing-off device and amplifier and automatically compensates for the potential difference generated at the electrodes. The device was tested in practice by placing an electrode and thermistor in each side of a U-tube. The temperature at one side was altered over a range of 7° on either side of the other arm.

It was found that over this range linear compensation was achieved, resulting in no detectable artifact. Details of the circuit are given in Fig. 3.

COMPARISON OF SKIN POTENTIAL AND RESISTANCE

As level of skin resistance has been used for many years as an index of such psychological concepts as 'energy mobilization' or more recently 'arousal', it was important, in order to be able to relate the work using skin resistance to that using skin potential level, to determine the relationship between the two measures. This was done in two studies.

The method used in both studies for the measurement of skin potential was substantially the same as that previously described, except that no temperature correction at the electrodes was made and 0·1 g. % potassium chloride was used as the electrolyte. (These two studies preceded the work described earlier in the paper.) The electrodes for potential measurement were placed on the palm and inner surface of the left arm. Resistance was measured by a conventional Wheatstone bridge circuit. The electrodes and electrolyte were identical with those used for potential measurement and were placed on the right palm and forearm. The current through the subject varied between 5 and 15 μA., depending on his resistance.

Owing to the disturbance in the measurement of skin potential created by the presence of the impressed voltage used to measure skin resistance, the two measures were not taken simultaneously. Skin resistance was measured first, the bridge voltage was then disconnected and the skin potential measurement made within 15 sec. Time was allowed for stable values to be reached, a process which took 10–15 min. on the average.

Forty male chronic schizophrenics took part in the first study and fifty-three in the second. Apart from a possibly greater range of electrical activity in schizophrenics there was no particular reason to think that the relationship between the two variables given by these subjects is not generally applicable in normal subjects. Plots of the two variables showed curvilinear relationships between potential and resistance. Conversion of the latter measure to conductance resulted in relationships which did not differ significantly from linear. In the first study where potential measures ranged from $-2{\cdot}0$ to $-40{\cdot}0$ mV. and conductance from 1·4 to 32·2 μmhos the correlation between the two variables was $+0{\cdot}68$, $P < 0{\cdot}001$. In the second study potential ranged from $+10{\cdot}3$ to $-47{\cdot}0$ mV. and conductances from 3·0 to 21·7 μmhos; the correlation in this instance was $+0{\cdot}51$, $P < 0{\cdot}001$.

It would thus appear that the two measures are related, although not so highly as to make them equivalent.

Wilcott (1958) did not obtain a significant relationship between potential and resistance measures. There would seem to be two possible explanations for this discrepancy. First, Wilcott had a larger number of subjects showing positive than negative potentials. The lower correlation in the second study above, compared with the first, is to some extent due to the low correlation between resistance and potential in those subjects with positive values of the latter. This apparent unreliability of subjects with positive skin potentials was confirmed in a later study, and is due to the low electrolyte concentration used (cf. previous section on electrolytes). Wilcott does not state what concentration of electrolyte he used. A second possible reason for the lack of correlation in Wilcott's study is that his normal subjects may have covered a limited part of the curvilinear relationship of potential and resistance where a high correlation would not be expected.

CONCLUSIONS

Some of the details which require particular attention when measuring skin potential have been described. Techniques have been advocated which keep errors arising from purely electrical artifacts to below 0·1 mV. The maximum artifact due to electrodes from drift and standing potential can be expected to be 0·1 mV. With adequate abrasion at the inactive site achieving a source resistance of 100 kΩ or less, the maximum artifact due to the passage of current is also 0·1 mV. Drift in the amplifier should be well below this figure. Artifacts due to temperature differences at the electrodes are adequately removed.

For comparison of potentials obtained from the same site on the same subject probably errors can be kept within 0·3 or 0·4 mV. However, when comparing subjects or the same subject on different occasions the equivalence of electrode sites is the main factor to be considered. Probably at least 1 mV. should be allowed as error due to differences in placement. This, however, is a figure which is difficult to determine and is only a guess which is based on the spread of potentials over sites used.

This method of skin potential measurement presents a scale for use in the comparison of subjects which probably has about fifty valid divisions and twice that number when comparing potentials on the same subject.

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MASSED PRACTICE AND REACTIVE INHIBITION, REMINISCENCE AND DISINHIBITION IN THE SPIRAL AFTER-EFFECT

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It is suggested that the rotating spiral and its after-effect may be used as a method of inducing and measuring reactive inhibition, or some perceptual parallel. Several related experiments are reported outlining the effects of massed and spaced practice and the reminiscence phenomenon resulting from the dissipation of inhibition, usually by rest. Stimulant and depressant drugs are shown to change the rate at which inhibition is generated, and also dissipated, in a predictable fashion. Another experiment outlines the way in which reactive inhibition generated by massed practice can be dissipated by an external stimulus. It is suggested that the occasional failure to generate reactive inhibition may be due to spontaneous dissipation by extraneous external or internal stimuli.

INTRODUCTION

Several authors have advanced explanations of the spiral effect, or of individual differences in perception of after-effects, which have been based upon or suggested by inhibitory hypotheses (Eysenck, 1955; Holland, 1957, 1958; Holland & Beech, 1958; Eysenck, Holland & Trouton, 1957; Eysenck & Eysenck, 1960). It was to test out predictions derived from such an inhibitory theory that the series of studies reported here was conducted. These studies are thought to be of added interest in that they were designed to test an hypothesis not hitherto widely applied to perceptual phenomena.

All the experiments in this report are concerned with aspects of massed practice, or its corollary reminiscence, and disinhibition. The first two indicate the attenuation of performance which is predicted of, and follows, massed practice, and the difference between massed and distributed trials. Expts. III and IV examine the improvement in performance following rest (reminiscence). Expt. V is also concerned with the reminiscence phenomenon, predicted by the drug postulate of the extraversion theory; the study is an attempt to demonstrate increases and decreases in performance due to the action of a stimulant and depressant drug. In Expt. VI an examination is made of the effect of a judgemental variable on massed practice and reminiscence. In the last study the effect of deliberate disinhibition by a distracting stimulus is demonstrated. None of the studies can be considered as definitive, and all reveal the complexity of an apparently simple perceptual task.

MASSED PRACTICE

If the spiral after-effect is, or is due to, an inhibitory state or process, it may be predicted that massing the trials, i.e. allowing no respite between them, should produce a progressive accumulation of inhibition and a progressive degradation of response magnitude. This prediction has been supported several times in the study of psychomotor phenomena (Eysenck, 1956; Rachman, 1962), but rarely in that of perception (Eysenck & Slater, 1958; Lynn, 1960).

Experiment I

The apparatus in this and subsequent experiments comprised a four-throw expanding logarithmic spiral (sometimes called the Exner spiral), driven at a speed of 100 rev./min. The spiral, which was 8½ in. in diameter, was viewed from a chin rest placed 6 ft. away (visual angle 6·5°). Illumination was provided by four 60 W. strip lights 14 in. long placed to form a square around the spiral disk. The lights were shaded, to prevent glare in the subject's direction, by a cover having an 11 in. diameter hole through which the disk appeared.

The subjects in Expt. 1 numbered 14: all were male, with ages varying between 15 and 18 years. None wore spectacles or reported impairment of vision when asked.

Procedure. Each subject was administered 35 massed practice trials with an inducing period of 30 sec.; i.e. the spiral was objectively rotated for 30 sec., and then stopped. The duration of the after-effect was assessed at the end of each trial between stopping the motor and the verbal report of the subject that the effect had 'ceased'. The report was taken as the signal for the end of the trial, and also the signal for the inducing period of the next trial to begin. The post-rotational gaze was directed on the inducing spiral.

Results. The results of testing can be seen in Fig. 1.

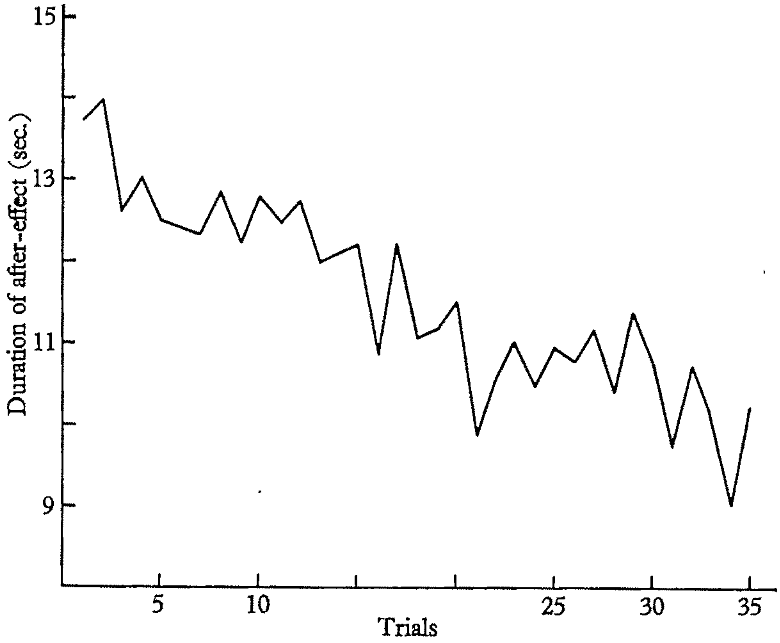

Fig. 1. Massed practice. Decrease in after-effect duration following massed practice. The graph shows the characteristic irregularity where large numbers of trials are employed. $N = 14$.

This figure outlines the general tendency for after-effect durations to diminish progressively with the number of trials. The trend, however, is far from smooth and, after an initial slight 'warm up', seems to become more uneven as the number of trials increases and the duration of the after-effect decreases. Despite its far from even constituent the general function describes a straight line with intercept at 13·32 sec., and a slope, calculated by the least mean square method, of $-0\cdot0840x$.

SPACED VERSUS MASSED TRIALS

To ensure that the progressive shortening of the after-effect was due to the massing of trials, a short study was conducted on a group of six young male subjects to outline the differences between 'massed' and 'spaced' trials.

Experiment II

The conditions and apparatus were the same as the previous experiment.

Procedure. The after-effect induction periods in this experiment were again 30 sec. in duration. There were 10 trials of each type, three of the subjects getting the order 'spaced-massed' and three 'massed-spaced'. The 'massed' trials were the same as those described under Expt. 1, the 'spaced' trials however had a 2 min. rest interval between them. There was a half hour break between the two types of trial series.

Results. The results of this experiment are shown graphically in Fig. 2, which delineates clearly the progressive decrease in after-effect values following massed practice, whereas spaced practice durations remain approximately the same from beginning to end. In this case the function of the massed trials results in an intercept at 13·31 sec., and a somewhat steeper decline of $-0{\cdot}250x$.

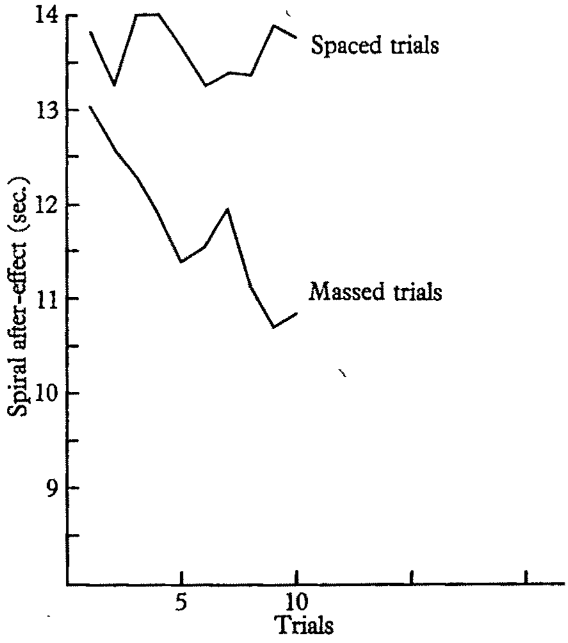

Fig. 2. Massed practice. Difference between massed and spaced practice in the same subjects. $N = 6$.

REMINISCENCE

A phenomenon often observed following massed practice is reminiscence (Ammons, 1955; Eysenck, 1956; Rachman, 1962). This is characterized by a return from an attenuated state to a full amplitude of response following a rest. (During the rest period it is assumed that any inhibition generated by the previous continuous reaction is dissipated.) In Hullian terms each trial adds an increment of reactive inhibition (I_R) which, under spaced practice conditions, dissipates during the inter-trial period. With no inter-trial period, however, as found in massed practice, reactive inhibition accumulates to degrade the response and to be dissipated in the rest interval which follows.

Clearly in this kind of experiment there are several factors which combine to give the optimal conditions for the appearance of reminiscence. These include, among others, the number of massed trials, the duration of each trial, and the rest period following them. (Reference is made to a number of tentative conclusions at the end of this paper.)

Experiment III

Procedure. In this experiment 33 subjects were administered 35 massed trials with an induction period of 30 sec. each. At the end of massed practice, the subjects were allowed to rest for 5 min. after which they were given three more identical trials.

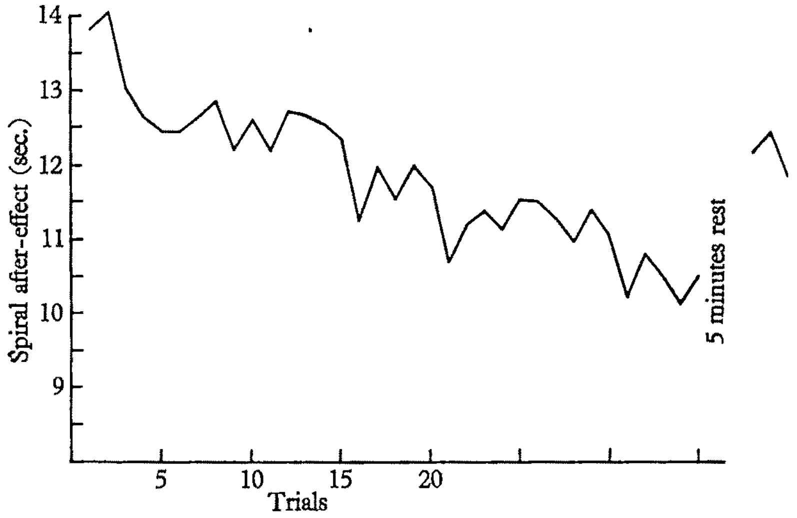

Fig. 3. Reminiscence. Fig. 3 shows the effect of 5 min. rest following 35 (30 sec.) massed trials. Reminiscence is assessed by the difference between the last three pre-rest trials and three post-rest trials. $N = 33$.

Results. The results are presented graphically in Fig. 3. This shows again that the initial 'warm up', already observed in Expt. 1, is followed by a considerable and progressive, if irregular, decline in scores, due to the massed trials, which in turn is followed by recovery after the 5 min. rest interval. The post-rest recovery, averaging about 2 sec. (approx. 56 % of total decline), is still a long way short of the starting level; a fact which suggests that the inhibition generated, or the fatigue developed, has not completely dissipated during the permitted rest.

Experiment IV

This experiment was conducted to determine whether a longer rest interval than the 5 min. allowed in Expt. III would lead to a more complete dissipation of inhibitory effects and a larger reminiscence.

Procedure. In this study, 14 people were presented with 20 massed trials of 30 sec. followed by a 15 min. rest interval. The rest interval was followed by a further 10 trials.

Results. The results are presented in Fig. 4 and Table 1. In this case the scores not only show the usual pre-rest outline, but that following the 15 min. interval there is complete recovery to the full amplitude seen at the beginning of the induction trials.

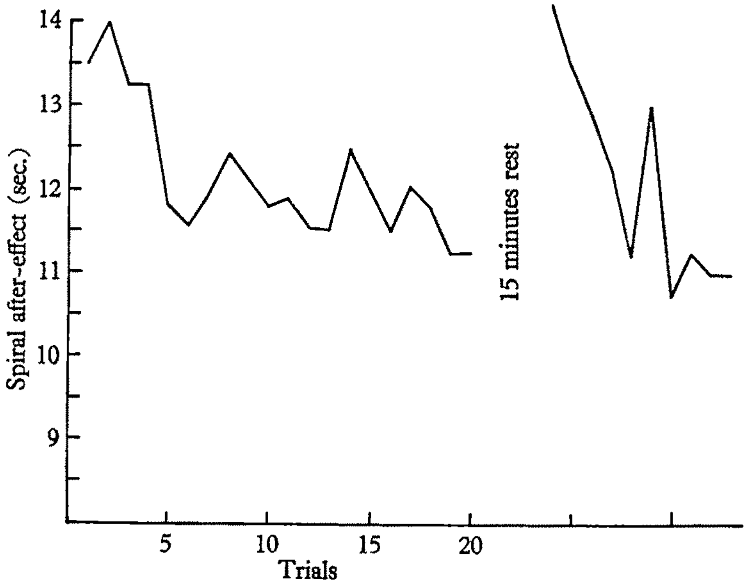

Fig. 4. Reminiscence. Fig. 4 outlines the scores in second reminiscence experiment. The curve indicates greater reminiscence following a longer rest period $N = 14$.

Table 1. *The progressive decline in scores and the temporary recovery following the* 15 *min. rest interval*

After-effect values		
Pre-rest trials		Post-rest trials
sec.	sec.	sec.
1. 13·5	11. 11·9	1. 14·3
2. 14·0	12. 11·6	2. 13·6
3. 13·3	13. 11·5	3. 12·9
4. 13·3	14. 12·5	4. 12·3
5. 11·8	15. 12·0	5. 11·2
6. 11·6	16. 11·5	6. 13·1
7. 11·9	17. 12·1	7. 10·8
8. 12·4	18. 11·8	8. 11·3
9. 12·1	19. 11·2	9. 11·0
10. 11·8	20. 11·2	10. 11·0

t, last three pre-rest trials *vs* first three post-rest trials, $= 32{\cdot}35$; $P < 0{\cdot}001$.

REMINISCENCE AND THE EFFECTS OF DRUGS

In recent years, the outcome of a number of experiments aimed at assessing the effect of drugs on behaviour has been predicted on the basis of an inhibitory-drug hypothesis which has been derived by Eysenck from extraversion theory. This is called by Eysenck the 'Drug Postulate', and puts forward the proposition that depressant and sedative drugs will increase inhibitory potentials and decrease excitation, whereas excitatory and stimulant drugs will increase excitatory potentials and decrease inhibition. Accepting this postulate it is possible to predict as follows:

If the decrease in spiral after-effect scores following massed practice is an inhibitory effect, and *if the reminiscence effect is due to the dissipation of inhibition*, then, *depressant compounds will lead to a more rapid degradation of responses and stimulant compounds to a less rapid degradation of responses than normally observed;*

also, *post-rest recovery (reminiscence) in the depressant drug condition will be greater in magnitude, because of the greater inhibition generated by preceding trials but not in proportion to the total decline, because of the slowed dissipation of inhibitory processes within this condition.*

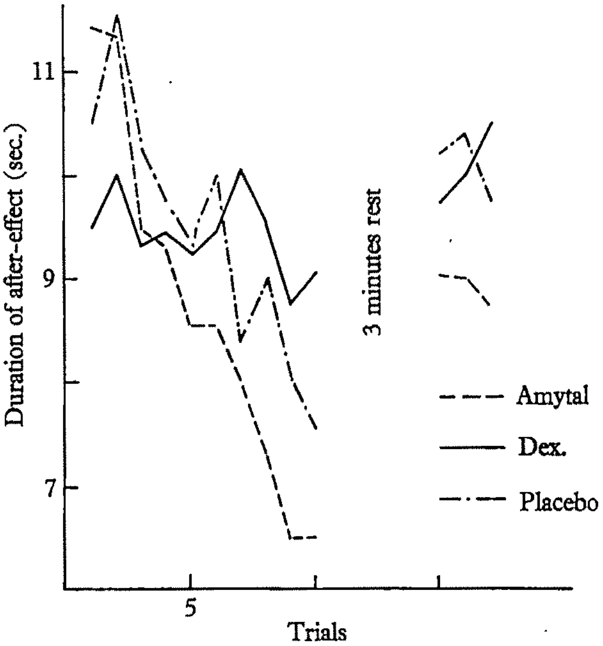

Fig. 5. Drug effect. The differential generation and dissipation of inhibition under the stimulant and depressant treatments, as well as the placebo. $N = 12$.

Experiment V

Procedure. Twelve subjects were seen on each of three days. They were given, on each day, one of three treatments: (*a*) 3 grains of sodium amytal; *or* (*b*) 10 mg. of dexamphetamine sulphate; *or* (*c*) a placebo. All treatments were administered in identical capsules. Testing commenced about 1 hr. after ingestion of the compounds. The order of treatments was randomized in accordance with four 3 × 3 Latin squares. In the experiment there were 10 massed trials of 30 sec. duration followed by a 3 min. rest interval. This short post-trial rest interval was chosen on the grounds that any developed inhibition was likely to be still diminishing by the end of it and consequently that it would permit the examination of the second portion of the two-part prediction.

Results. The results may be seen by reference to Fig. 5 and Table 2. The graphic presentation in Fig. 5 confirms in a striking way all the particulars of the prediction as stated: namely, that there is a more rapid development of inhibition under the depressant condition than under the stimulant condition, and that the reminiscence magnitude, although greater in total amount under amytal than under dexedrine (i.e. 2·17 sec.), is smaller in proportion to the recovery of the dexedrine group, which is almost complete. Following 3 min. of rest the recovery in the amytal group is only about two-thirds of the preceding decline in score, whereas the recovery of the dexedrine group is back to full early trial magnitude. Table 2 presents an outline of the analysis of variance conducted upon the average scores for the drug conditions.

Table 2. *Outline of analysis of variance on mean trial scores for the three treatments*

Source	d.f.	S.S.	M.S.	F	P
Between subjects	11	4221·56	383·78	48·09	$<0·01$
Between drugs	2	64	37·07	4·02	$<0·05$
Between trials	14	375·00	26·79	3·36	$<0·01$
Trials × drugs	28	147·51	5·27	—	N.S.
Residual	413	3293·90	7·98	—	—
Total	468	8102·10	—	—	—

$t\ AB, = 2·47; P < 0·05$
$t\ AC, = 2·46; P < 0·05$
$t\ BC, =$ —.

(A, amytal; B, placebo; C, dexedrine.)

Table 3. *Outline of analysis of variance of the differential decline scores for the treatment conditions*

Experiment V. Analysis of scores representing the effect of massed practice (i.e. trials $1+2+3-8+9+10$).

Group means in second of decline scores for the three treatment conditions.

Amytal	Dexedrine	Placebo
11·46	1·91	7·63

Outline of analysis of variance of decline scores

Source	d.f.	S.S.	M.S.	F	P
Between people	11	487·35	44·31	1·33	N.S.
Between drugs	2	826·20	413·10	12·39	$<0·01$
Residual	22	733·68	33·35	—	—
Total	35	2047·23	—	—	—

t, amytal – dexedrine = 3·80; $P < 0·01$
t, amytal – placebo = 1·54; N.S.
t, dexedrine – placebo = 2·26; $P < 0·05$

The decline in scores from the beginning to the end of the massed trials has been assessed in a very simple way, namely, by substracting the last three trials from the first three. The results of the analysis of these decline scores are presented in Table 3. This table indicates that the decline of after-effect duration under the stimulant drug condition averages only 0·69 sec. whereas the depressant has resulted in a reduction averaging 3·8 sec.—over five times greater. The placebo lies mid-way between these scores with an average decline of 2·54 sec., and must be assumed, if suggestion is discounted, to represent the normal decline in performance due to all causes. The over-all rate of decline is clearly different as the graph (Fig. 5) indicates, a differentiation which, however, is confined to the comparisons amytal *vs.* dexedrine and placebo *vs.* dexedrine, when examined by *t* tests following analysis of variance.

JUDGEMENTS AND REMINISCENCE

Anyone carrying out spiral experiments is soon impressed by the different ways in which subjects judge the after-effects to have 'ceased', some exercising great caution and deliberation whereas others make what can only be called 'snap' judgements. Impressive, too, is the very wide range of after-effect scores (Holland, 1960), and the ease with which they can be changed by varying instructions (Holland, 1961). Because of these features, it was decided to examine the hypothesis that *differences in the method of judging the end of the effect would lead to differences in the length of the after-effect, and to a differential development of inhibitory effects and thus to differential reminiscence.*

Experiment VI

Procedure. Some 16 subjects were tested using the previously described technique of massed practice, i.e. ten 30 sec. trials followed by 5 min. rest, followed by three more trials. The subjects were young males between the ages of 16 and 18. After testing, the average score of the 10 pre-rest trials was determined for each subject and two subgroups established, the first having an

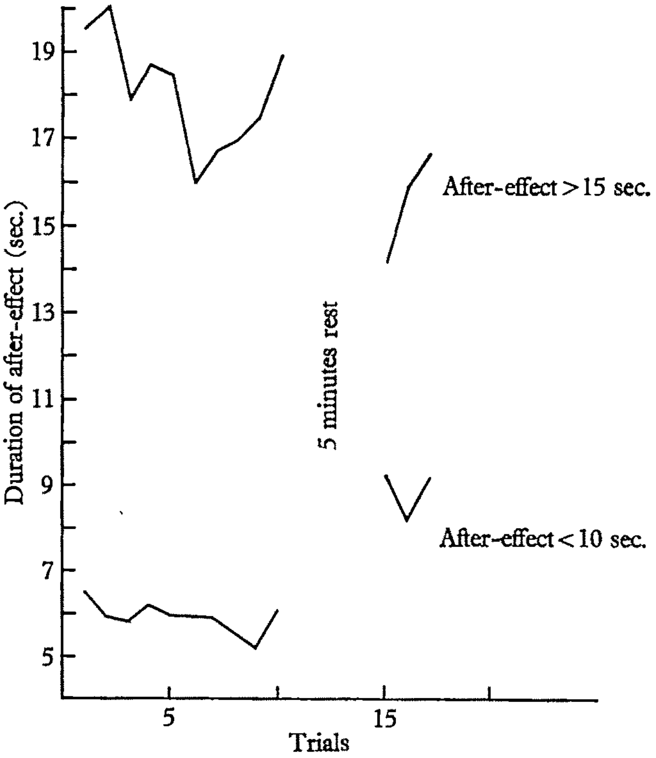

Fig. 6. Judgements. Differences in reminiscence in two groups representing (*a*), those with after-effect durations greater than an average of 15 sec., and (*b*), those with after-effect durations of less than 10 sec. Group 1 = 6, group 2 = 5.

average spiral after-effect duration score of less than 10 sec. and the second group achieving an average score of more than 15 sec. There were 6 subjects in the first group and 5 subjects in the second.

Results. Fig. 6 outlines the curves under discussion for the two groups. The group with the larger scores varies more erratically than the others but there is little to choose between them in the mean downward trend. The main difference between the groups appears to be in terms of reminiscence. The group with after-effect scores averaging above 15 sec. has no reminiscence effect, but what in fact appears to be a non-recovery or negative reminiscence, whereas the group with the shorter duration has a very profound increase following the 5 min. rest.

On such small samples of subjects these results can hardly be more than 'interesting', but considered in relation to other studies which have shown a high correlation between introversion and long spiral after-effects (Holland, 1959), and the legendary greater 'cautiousness' of introverts, at least two speculations seem permissible. First, that the longer after-effect represents a general 'caution' which is a psychological correlate of inhibition, and secondly that the longer after-effect judgement represents for the individual a method of 'handling' or compensating for inhibition and its effects. Kimble (1949) has shown that reminiscence may be absent if rest periods of only a few seconds are permitted to occur between trials. This being so, the longer after-effect may provide for the subject a means of maintaining continuous performance at a near optimal level rather than suffering the fractionation of response due to the neurological parallel of fatigue, i.e. inhibition.

Disinhibition

The evidence so far presented suggests that massed trials upon the spiral lead to progressive reduction of scores, due to the growth of inhibition which is dissipated under spaced trials during programmed intervals of rest or during voluntary or involuntary rest periods. *Inhibition, however, is not only dissipated by rest, but can be temporarily removed by the introduction of an alien or novel stimulus* (Pavlov, 1927).

Experiment VII

Procedure. In this experiment, 22 subjects were administered 20 massed trials, 30 sec. each in duration. The trials were followed by 5 min. rest which was, in turn, followed by a further 3 massed trials. Superimposed upon this routine were 2 disinhibiting trials. In the first group these trials occurred at trials 18 and 19; in the second group at trials 8 and 9. The disinhibitory trials were so called because for 15 sec. of the 30 sec. induction period (from the 10 sec. to the 25 sec. points) an electric bell was sounded. The bell was out of sight of the subject, who was instructed to ignore it.

Results. The results are again presented graphically in Fig. 7. As will be seen, the decline in value of the after-effect scores is arrested in each case by the introduction of the bell and there is a return to almost full (i.e. non-fractionated) response. This recovery is of course much greater when disinhibition is delayed until late in the series as more inhibition has been generated by the larger number of massed trials.

The second method of assessing the disinhibitory value of the 'alien' stimulus is by the degree of reminiscence which follows the massed trials, the prediction being that the late dissipation of inhibition by a disinhibitory stimulus would lead to little if any inhibition remaining to be eliminated by rest during the post-trial rest interval. The obverse of this argument is true for the earlier disinhibition, as the large number of remaining trials would tend to regenerate inhibition, depress the response, and lead to the possibility of considerable reminiscence.

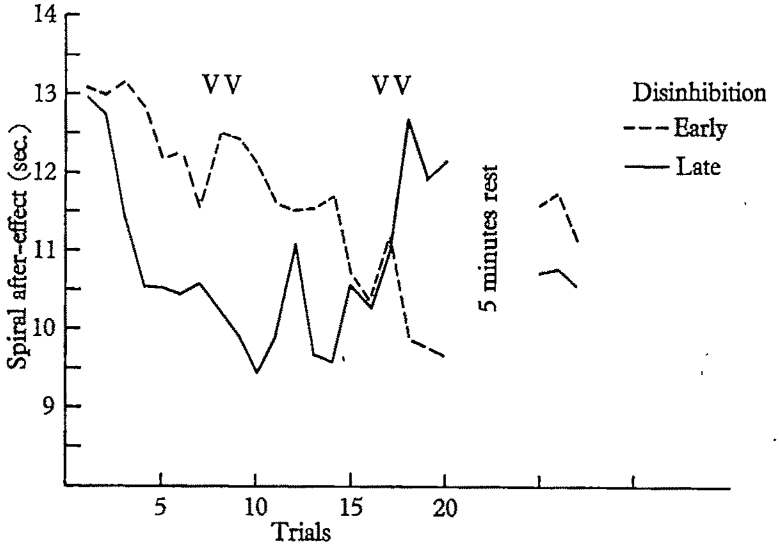

Fig. 7. Disinhibition. The spiral after-effect duration and reminiscence following disinhibition by alien stimulus at early and late points in a massed practice series of 20 trials. $N = 22$.

The expectation concerning differential reminiscence is fulfilled as the post-rest trial values indicate. Being only temporary, disinhibition due to the bell is followed by the subsequent accumulation of inhibition in the remaining trials and, if there are many trials as in group 2, the accumulation is considerable and the reminiscence effect proportional. Alternatively, if disinhibition occurs towards the end of the massed trials, as in group 1, the subsequent accumulation is negligible and any reminiscence effect proportional. Both effects can be clearly seen in Fig. 7.

Conclusions

On the basis of the evidence presented in the preceding experiments it is clearly reasonable to accept that perceptual tasks are as subject to the effects of reactive inhibition as other response processes.

Expts. I and II outline the difference in response (i.e. spiral duration) between massed trials, when no rest interval is permitted between the end of one trial and the beginning of the next, and spaced trials, when an inter-trial rest period is programmed into the series. These experiments delineate the declining trend of the response due to massed practice but draw attention to its irregularity after the first half dozen trials. The reason for the irregularity can only be speculative but clues are forthcoming from subsequent experiments which suggest that the 'spontaneous recoveries' which underlie the unevenness of response may be due to temporary disinhibition by extraneous stimuli from either the external or the internal environment. Alternatively, such disinhibition may spring from some personal tempo adopted by the individual, on either a temporary or a permanent basis, to 'pace' himself and prevent fatigue effects of all kinds. Probable examples of the former which spring to mind immediately are the 'psycho-sensory restitution phenomenon', described by Lowenstein & Lowenfeld (1952*a*, *b*), or the 'involuntary' rest pause, described by Eysenck (1956), both of which are clearly processes of disinhibition. Any alien stimulus from whatever source is likely to lead to dissipation of the accumulated inhibition by 'reminiscence', normally only achieved by rest. Examples of the latter variety of control of inhibition (i.e. personal tempo) probably underlie a number of individual differences.

The 'reminiscence' phenomenon, as applied to the spiral after-effect, is dealt with more specifically in Expts. III and IV, where the influence of the number of massed trials and the post-trial rest interval are outlined, as are the effects of a stimulant and depressant drug on the length of after-effect. The experiments conducted lead to the following tentative conclusions: (*a*) there must be sufficient trials to generate undissipated inhibition; (*b*) the rest interval between sets of massed trials must be long enough to permit the complete dissipation of any accumulated inhibition; and (*c*) the individual trials should be short enough for 'attention' (in the widest sense) to be maintained. Expt. V adds support to both the 'drug postulate' and to the hypothesis that the spiral after-effect is itself an inhibitory phenomenon or an indication of it. The relationships outlined by Fig. 5 go further than this, however, suggesting also the more rapid development of inhibition under the depressant drug conditions and its resistance under the stimulant. The fact that the reminiscence effect is proportionally larger under the stimulant drug treatment suggests the slower dissipation or longer retention of inhibition effects under the depressant drug. These results are thought to be of particular interest as, with one exception, namely the starting level, they are precisely in line, not only with the drug postulate but with the inhibitory characteristics claimed in the Extraversion Theory (Eysenck, 1957).

Arising out of Expt. VI is the interesting finding that subjects with relatively long spiral after-effects have minimum or negative reminiscence values, whereas those with shorter after-effects produce significantly higher post-rest scores. A second aspect lies in the appearance of what must be called a 'negative reminiscence effect', i.e. a worsening of scores following a rest interval. It can only be assumed that this effect is due to some decay of perceptual set, some loss of skill, or some form of forgetting.

Finally, in Expt. VII the question of deliberate if temporary disinhibition by an alien stimulus has been examined. The experiment shows not only that the introduction of an external and extraneous stimulus leads to dissipation of inhibition and a return to optimal or near optimal performance, but also that disinhibition towards the end of a series of massed trials reduces or removes reminiscence. In this experiment 'late' disinhibition led to a second example of a negative reminiscence effect. There is perhaps a clue here which suggests that post-rest decrement in performance may occur only when inhibition has been largely or completely dissipated, spontaneously or otherwise, prior to the rest interval.

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INTELLIGENCE, SEASON OF BIRTH AND CLIMATIC TEMPERATURE

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Among a sample of adult intellectual subnormals, season of birth is significantly associated with the subsequent level of intellectual performance. An analysis of these data was made, together with climatic temperature levels during the embryonic development of these individuals. The association between season of birth and subsequent intellectual performance appears to be a result of the climatic changes (as measured by climatic temperature) occurring throughout pregnancy.

A previous report (Orme, 1962) confirmed earlier accounts that season of birth was significantly associated with an individual's subsequent intellectual performance. Winter and spring births gave adverse performances compared with summer and autumn births. Varying suggestions occur in the literature as to the critical period of development involved (Orme, 1962). The present report attempts to examine these hypotheses using a random sample of adult patients in mental subnormality hospitals in the Sheffield area. All are in the 40–69 I.Q. range, the I.Q. measurements being derived from Coloured Matrices performances (Orme, 1961). The latter performances are used in an attempt to measure the intellectual efficiency of the organism, relatively unaffected by educational or manipulative factors.

The most obvious seasonal climatic variable is that of temperature. Most of the subjects studied were born in the Sheffield area and temperature data for this area were available. It was argued that the seasonal effect on intelligence would be paralleled by the effect of monthly temperatures above or below that month's average temperature. It was further argued that such a finding would be restricted to a certain period during development if that period was a specially critical one for intelligence. For each individual the month of birth and the preceding nine months were examined. This covered the period from conception to birth.

The present sample of 188 is the one used previously (Orme, 1962) with the addition of 40 more cases. Table 1 gives the seasonal distribution of births contrasting the third (55–69 I.Q.) and fourth (40–54 I.Q.) standard deviations below the mean. The previously reported seasonal effect persists, with a higher level of statistical significance because of the increased sample size.

Table 2 shows that apart from this seasonal effect, intelligence is enhanced by the number of months of embryonic development with a mean monthly temperature greater than that month's over-all monthly mean temperature. Taking the seasons separately, only summer birth individually repeats this effect significantly. No one peirod or month of embryonic development produced evidence of special importance using this method.

On the other hand, certain calendar months or sequence of months appeared to produce significant effects. These are listed in Table 3. These effects involve the fourth to eighth month of pregnancy, the only common pregnancy month being the fifth one.

The three possibly critical periods suggested in the literature (see Orme, 1962) are not specially indicated here.

There have been certain climatic changes this century, but these cannot indirectly account for any of the present results. Table 4 shows no significant difference between the higher and lower I.Q. groups for year of birth.

Table 1. *Season of birth and I.Q. incidence* ($N = 188$)

	Summer (May–July)	Autumn (Aug.–Oct.)	Winter (Nov.–Jan.)	Spring (Feb.–Apr.)
Mean temperature (°F.), 1883–1950	56·5	55·5	40·9	42·3
No. of births, I.Q. 55–69	31	23	15	21
No. of births, I.Q. 40–54	15	22	32	29

	χ^{2*}	D.F.	P
Four seasons separately	12·38	3	< 0·01
Summer/autumn *vs.* winter/spring	9·29	1	< 0·01
Summer *vs.* winter	9·77	1	< 0·01
Spring *vs.* Autumn	1·07	1	N.S.

* All 2×2 χ^2 are calculated with Yates's correction.

Table 2. *Monthly climatic temperature during pregnancy*

	Number of months: Above mean	Number of months: Below mean	χ^2	P
	(*a*) Total sample			
I.Q. 55–69	504	396	7·44	< 0·01
I.Q. 40–54	486	494		
	(*b*) Summer births			
I.Q. 55–69	186	72	5·30	< 0·05
I.Q. 40–54	124	78		

Table 3. *Significant calendar month effects during pregnancy*

(*a*) Spring births. November temperatures only

	Above average	Below average	χ^2	P
55–69 I.Q.	15	6	6·69	< 0·01
40–54 I.Q.	8	21		

(*b*) Summer births. January, February, March temperatures

	All 4 months below average	Remainder		
55–69 I.Q.	3	121	6·36	< 0·05
40–54 I.Q.	8	52		

(*c*) Autumn births. May to August temperatures

	No. of months above average	Remainder		
55–69 I.Q.	55	37	3·77	N.S.
40–54 I.Q.	39	49		

Summer and autumn birth, therefore, appears advantageous to subsequent intellectual performance compared with winter and spring birth. Summer and winter are clearly the maximum and minimum points. This effect is not simply due to the difference in temperature between these two seasons. The present results suggest that the effect is associated with the trend of climate throughout the whole of pregnancy. Summer births do not have an advantage with regard to intelligence solely because of the increased temperature in summer—but because of the increasing temperature in winter and spring to summer during pregnancy.

Table 4. *Incidence of year of birth*

	I.Q. 55–69	I.Q. 40–54
Before 1910	27	23
1910–19	22	25
1920–29	13	25
1930–39	15	14
After 1939	13	11

The converse applies to winter births and the decrease in temperature in summer and autumn to winter. It is just possible that the present results indicate that the middle period of pregnancy is specially vulnerable, but this might only reflect again the detrimental effect of relatively cold winters and springs or the beneficial effect of relatively warm summers and autumns.

This effect of seasonal climatic change on subsequent intellectual performance must be persistent to be demonstrable in adults. This is particularly so considering the relatively crude temperature measures used here. Furthermore, intellectual test performance scores are themselves more of an estimate than a precise measure. In addition it is not possible to identify subjects in the present sample who would presumably obscure this seasonal effect. Intellectual subnormality can be caused or increased by factors such as brain injury arising from accident or infection. A recent study (Greenberg, 1963) has claimed a significant association between low climatic temperature 7–8 months before birth and the incidence of mongol births to young mothers. As mongolism results from a chromosomal abnormality, any climatic effect on its incidence must be related to changes before embryonic development. Greenberg suggests that his data would therefore agree with evidence that mongol births tend to prematurity.

One further point is the indirect validation of the present intellectual performances as being measures of a person's fundamental ability regardless of subsequent environmental hazards. It might be argued that the seasonal effect is enhanced in the intellectually subnormal as they are especially vulnerable, but the literature provides similar results for normal ranges of intelligence and one can only presume the causes are similar.

As studies reporting a seasonal effect on intelligence come from different countries it seems that this effect is related to local climatic changes and not to any fixed temperature levels. But, obviously, similar studies to the present one in countries with different mean levels of temperature and with different seasonal variations of temperature would be of great interest. Seasonal studies have been unduly neglected, although

Fitt (1941), Cattell (1957), and others, have shown seasonal variations of psychological and physiological properties apart from the initial effect of season of birth. Seasonal climatic changes, in fact, offer an experimental design on a grand scale. With regard to the specific problem of intelligence, little is known of a precise nature concerning its determinants. The present findings suggest one area worthy of more attention.

The author wishes to thank the Sheffield City Meteorological Department for allowing access to their records.

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PUBLICATIONS RECENTLY RECEIVED

Inhibition and Choice: A Neurobehavioral Approach to the Problems of Plasticity in Behavior. By SOLOMON DIAMOND, RICHARD S. BALVIN and FLORENCE RAND DIAMOND. New York and London: Harper and Row. 1963. Pp. viii + 456. 52*s*.

This book represents a further stage on the road towards the contemporary physiologization of American psychology which many thought began with 'The Organisation of Behavior'. But the fact that Hebb's book is only referred to tangentially reflects the exciting advances in neurophysiology in the last decade. As Hebb himself admits, the epoch-making work of the Magoun school on the reticular arousal mechanisms knocked the props away from the theoretical scaffolding he had so carefully constructed. Even as we were studying Hebb and admitting the new edifice he had created for us, the demolition of the foundations had begun!

But though the present authors lead us farther along the road to a better understanding of the neurophysiology of behaviour we reach no definite goal: explicitly they do not set out to arrive but rather to smooth the journey by stressing two important aspects. Inhibition, they firstly contend, has been neglected by psychologists or only grudgingly recognized, and then as a nuisance, whereas the evidence they marshal for it being a physiological fact is impressive. Across the pages of their illuminating historical review march the familiar great, like Pavlov and Sherrington, Wundt and Head; the less familiar great like Sechenov and Hedenhain; and the undeservedly unfamiliar great like Brunton and Anstie—the British physician whom we might, it is clear, justifiably regard as a father of psychopharmacology. Each in turn contributes to Diamond, Balvin and Diamond's theme that inhibition is more than 'the conceptual opposite of a positive response tendency'. Their view of the centrality of inhibition is well underlined by their approving quotation (twice) of R. Jung's telling description of the brain as 'the enormous synaptic powder barrel we carry in our heads' ready to explode in epilepsy were it not for inhibitory influences.

The historical section occupies over a third of the book and intermeshes with the development of the authors' second major theme. This is no less than that behaviour is choice, and that it occurs 'by the modulation of inhibitions, and not by the excitation of specific acts'. The evidence for this position is less satisfactory, though their surveys of various fields (learning theory, mental retardation, conflict behaviour and drug action) from this point of view enable them to formulate new insights in many directions. Some of these are more convincing than others and it is perhaps indicative that this reviewer found them more acceptable in proportion to his unfamiliarity with the field in question.

Just as the authors stress the plasticity of behaviour and the manifold avenues of possible response, modulated in the outcome by inhibition of some, and disinhibition of others, so they would have us keep our theoretical notions in similar flux. This is a counsel, not of perfection, but of plasticity. Occam's razor, they contend, is a double-edged weapon and may have done more damage to its users than to the problems it has been wielded against. Thus their plea for *less* parsimony ('almost like taking a stand in favour of sin'!) is concerned with mitigating the 'inevitable embarrassment of any (learning theory) attempt to deal with inhibition as an aspect of a response-generated mechanism'.

This lively and stimulating—or perhaps I should say 'disinhibiting'—book is pleasantly free of typographical errors (though Pribram's name is consistently misspelled) and its careful scholarship is reflected in an excellent bibliography and comprehensive indices.

P. L. BROADHURST

Conditioning and Learning. By ERNEST R. HILGARD and DONALD G. MARQUIS. Revised by GREGORY A. KIMBLE. London: Methuen. 1961. Pp. xii+590. 60*s.*

It is rather startling to find that this first revision of Hilgard and Marquis is so extensive that we have what amounts to a new book on conditioning by a new author (Kimble). In part this reflects the enormous expansion of work on conditioning during the two decades since the original edition appeared. With a mass of new material clamouring for inclusion, Kimble has found it necessary to narrow the scope of the new edition by omitting the chapters on serial learning, problem solving, voluntary action and neurophysiological mechanisms—although these topics are not wholly neglected. (As a supplement to the neurophysiology in the book, the recent review on the neurophysiological basis of memory by Deutsch in the *Annual Review of Physiology*, 1962, can be recommended.) Kimble states that his revision is like the parent edition in being basically empirical rather than theoretical in approach. However, a considerable amount of theory is included. Moreover, Kimble is clearly influenced by his own neo-Hullian position both in his discussion of theoretical issues and in his selection of material. He is very concerned to show that, at the empirical level, there is no such thing as a non-reinforcement theorist (p. 203); even Tolman is regarded as a reinforcement theorist. He writes (p. 209): 'The terms *significate* and *goal* are the terms referring to reinforcement in expectancy theory. Learning is assumed to occur whenever a sign is followed in time or space (that is, reinforced) by some other signified object'. The term *reinforcement* seems to be used so widely here as to lose all its meaning. For what can there be to learn unless one event follows another in time or space? Even his definition of learning refers to reinforcement: 'Learning is a relatively permanent change in behaviour potentiality which occurs as a result of reinforced practice.' Such a definition seems to prejudge many issues. For example, in addition to assuming that reinforcement is necessary for learning, it excludes the possibility of transient learning and it also suggests that learning cannot be single-trial.

Perhaps the main attraction of this book is that it provides a fairly up-to-date survey of experimental work on conditioning. Inevitably the survey is selective (for example, British workers not mentioned include Deutsch, Trotter and Hinde), but a comprehensive survey would have required a book several times longer. Some may complain that work emanating from Kimble's own laboratory has a disproportionate place. Yet it is not a bad thing for an author to write at somewhat greater length about the work with which he is most familiar. In general the author tends to accept experiments at their face value rather than be critical of them, but perhaps he might still be writing the book if he had been too ambitious. As it is, the book represents a considerable achievement on the part of a single author.

Certain topics are omitted or receive less attention than might have been expected. Little is said about schedules of reinforcement (merely listing some of the main schedules which have been tried is not very informative). Statistical learning theory is barely mentioned. There is no explicit reference to the use of probability models either for descriptive purposes or for testing hypotheses about the course of learning or extinction: the reader is left with the impression that abstract over-all measures of behaviour are perfectly adequate. (Bush and Mosteller are mentioned, but only for their definition of learning.)

Some of the limitations of this book have been indicated above. But whatever its limitations it provides both for students and others a very useful mode of access to a large slice of the literature on conditioning.

JOHN BROWN

Explorations in Cognitive Dissonance. By J. W. BREHM and A. R. COHEN. London and New York: Wiley. 1962. Pp. xvi+334. 60*s.*

This work extends Festinger's *Theory of Cognitive Dissonance* published in 1957. His thesis, in short, was that when one set of cognitive elements follows from the obverse of another set, a state of dissonance exists, and that psychological events will occur which will reduce the dissonance. As Brehm and Cohen make clear, however, unless one set of elements can be held constant the subsequent events can often be interpreted in terms of other theories, such as the usual judgmental processes or conflict-resolution. Festinger's theory owes its uniqueness to its restricting itself to instances where that constancy holds; in particular to instances in which the subject has 'committed' himself and cannot back out of one set of cognitions. Within that restriction the theory makes successful predictions which are 'nonobvious' and run counter to common sense and to the implications of some learning theories. The subject may have committed himself to expressing publicly a view with which he privately disagrees. The theory predicts that the smaller the reward offered to induce the commitment, the greater will be the dissonance, and therefore the greater will be the probability of the subject's privately accepting the view he previously rejected. Brehm and Cohen provide much new evidence on this kind of 'commitment' situation, and also attempt to apply the theory to new areas, including Motivation, Personality, and Social Influence. For example (motivation), will the subject's commitment to continue a task in spite of hunger reduce his cognition of hunger?

The experiments quoted are ingenious in design and conception, and the deductions drawn could be far-reaching. Because of the purpose of the book, the arguments are closely reasoned, and the reader will find that the text cannot be skimmed. Will his effort be worth while? The authors themselves admit that some of the results can be interpreted in terms of other theories–which they examine. In this respect your reviewer's judgement is favourable. He believes that further explorations, particularly into the circumstances in which different modes of dissonance reduction are adopted, will increase our insight.

JAMES M. THYNE

Signs, Signals, and Symbols. Edited by STELLA E. MASON. London: Methuen. 1963. Pp. xii + 212. 35*s*.

An interesting, rather pretentious exercise in professional higher education, faintly motivated by the desire to keep up with the Joneses. A kind of high-minded 'commercial' designed to enhance the public image of a technology, that has no very obvious mystique, like medicine, and no very powerful mass lobby, like teaching. Both of these laudable aims seem to have been in the minds of the organizers of the 1961 National Conference of Speech Therapists, from which the thirty-two papers (and one introductory address by the Bishop of Birmingham), collected together in this attractively produced volume, are derived. But the aims are not wholly compatible, and the contents of the packet do not quite measure up to the king-size wrapper of communication theory with which they are provided.

The whole is somewhat chauvinistically presented as 'a British approach to Speech Pathology and Therapy', our own linguistic deterrent perhaps. Speech therapy, so runs the protestation, is a science (Dr Stein *et al.*, *passim*) 'dealing with what is empirically given by means of logical operations alternating with observation, description, arrangement, prediction, and experimental observation'. The conceptual underpinning, apparently, is to be provided by communication theory. So we get an excellently succinct statement by D. B. Fry about *Coding and Decoding in Speech*—useful to any student of verbal communication—and two very good papers by Haas of Manchester and Trim of Cambridge on *Signs and Signals* and *Linguistics and Speech Pathology*. There is, too, an apocalyptic meditation by Meredith of Leeds on *The Quantum of Language*, clothed in space, time, and relativity, mystic, wonderful, and leading up to a lysergic penetration of civilization itself.

These hors d'oeuvres are something of a meal in themselves; the other papers provide a homelier main dish, a mixed grill widely representative of the everyday practice of the speech clinic. Some of the papers are useful fillers, the harmless vegetable matter that goes to make up a conference. There is some helpful practical advice about instrumentation and diagnostic aids. There are examples of not very highly seasoned quasi-psychiatric, pseudo-analytic mystification that are difficult to swallow, much less digest. There are three striking papers from Nottingham by Simms, Hartley and Grady on the phonological aspects of dyslalia.

Obviously the book is uneven, but there is a good deal that should be interesting, not only to the speech therapists, but also to their neighbour disciplines. It is interesting to know what they are up to, it is entertaining to learn what they would like to be up to, but I suspect that it would set them back on their heels if we really started to refer to them as Rhematologists.

GEORGE SETH

The Fundamental Forms of Social Thought. By WERNER STARK. London: Routledge and Kegan Paul. 1962. Pp. x+270. 32*s.*

Theory of Collective Behaviour. By NEIL J. SMELSER. London: Routledge and Kegan Paul. 1962. Pp. xii+436. 45*s.*

Human Behaviour and Social Process. An Interactionist Approach. Edited by ARNOLD M. ROSE. London: Routledge and Kegan Paul. 1962. Pp. xvi+680. 56*s.*

These three books raise directly or indirectly various basic problems for the social sciences and psychology. It is, for instance, all too easy to demand that psychology and sociology be 'scientific', and that human behaviour and action be viewed with the same eye as the non-human world. The undeniable fact is that we can and do 'understand' human actions in a way in which we cannot 'understand' any non-human phenomena. One may, of course, take the view that science has simply nothing to do with such 'understanding', which can be relegated to 'common sense'. But in this case, it may be argued that we have an important aspect of human behaviour which is beyond the reach of science altogether and yet remains a distinguishing feature of the phenomena with which we profess to be concerned. May this simply be that we are *defining* science in terms of the non-human sciences and then requiring the scientific study of human behaviour to conform to this model? As Dr Stark suggests, we may, in our attempts to do this, be simply doing the opposite of what was done by much earlier human beings when they tried to treat natural phenomena in terms appropriate only to human beings. If we refuse to concern ourselves about this dilemma, we do not thereby get rid of it. It could not reasonably be implied that the application of 'scientific' thinking on the analogy of the physical sciences has not yielded and will not continue to yield great progress in our knowledge of human beings and their ways. The question is, is this enough? Is this *all* that psychology and the social sciences can do?

Another kind of problem is that of the 'levels' of explanation. Sometimes we hear of the 'integration' of psychology and sociology. But it can plausibly be argued that psychologists and sociologists are not asking the same kind of questions nor using the same 'perspective', and that one cannot look at human phenomena from two perspectives at once, though one can move back and forward, as it were, between them. Durkheim, although he is sometimes difficult to follow and tends to overstate his case, appears to have been basically correct in his contention that we cannot explain 'social facts' (in other words, answer *sociological* kinds of questions) purely in terms of 'individual facts' (in other words, in terms appropriate to the answering of *psychological* kinds of questions). Sometimes we get into difficulties because the same or similar terms are used in very different senses. For example, it is clear that when a sociologist speaks of 'motivation', he may be meaning something very different from the psychologists, and this may be very confusing. This difference in usage, however, corresponds to a difference in perspective. Very often, though by no means always, the sociologist is concerned with specifying the social *conditions* under which various forms of social action occur, while the psychologist is concerned with the adaptation of individuals to given conditions. Thus the sociologist's account of certain kinds of social behaviour is not likely to satisfy the psychologist and vice versa. Psychologists, especially those concerned with 'culture and personality', have often got themselves into trouble because they have not observed the limits of the questions which they may legitimately attempt to answer; while sociologists have, of course, committed the corresponding errors.

Only one of the three books referred to here can be considered immediately relevant to psychology, and that is Dr Rose's volume. The other two books are strictly sociological, Dr Stark's, indeed, bordering upon social philosophy. Dr Stark reviews the writings of well-known social theorists, in so far as these make use of one (or more) of three kinds of model for thinking about society—the organismic or biological model (social realism), the physical or mechanical model (social nominalism) and the 'process' model. In the first group come such writers as Spencer, Trotter, Schäffle and Lilienfeld. It is pointed out that anthropologists, especially of the functionalist school, have tended to organismic thinking. In the second group come writers like Rousseau and in particular Pareto. As one of the 'secondary forms' of mechanism, the physicalism of George Lundberg and Stuart Dodd has three pages devoted to it. Lundberg receives most of the attention and Dr Stark deals severely but, in the reviewer's opinion, justly with him.

Again, it is pointed out that the classical and neo-classical economists have tended to follow the mechanistic model. The *Social* bases for the predominance of one or the other of these types of social thinking at certain times and in certain writers are analysed in a very lucid way. Finally, Dr Stark sees in the view of society as 'process', as expressed in the writings of Fouillée, Tönnies, Simmel and especially in the 'cultural sociology' of Cooley and Sumner, the reconciliation of the two opposite views of realism and nominalism. Dr Stark's second-last chapter considers 'the persistence of the traditional dichotomy' in such writers, in particular, as Durkheim and Max Weber. While both are properly to be regarded as members of the 'cultural' school, Durkheim emphasized the *collective* factor, thus tending in the direction of organicism, while Weber 'insisted that collectivities are merely collections of individual men' and thus tended in the direction of mechanism. Again in these two cases Dr Stark indicates reasons for these tendencies in the social background and position of the two men.

Although not concerned directly with psychology, Dr Stark's book is one which psychologists might do well to read and ponder. It is very clearly written; from the point of view of the development of social theory, it is highly illuminating; and the discussions of writers in terms of the sociology of knowledge suggest that the same kind of questions could just as interestingly be considered in the case of psychological theorists.

Dr Smelser's book on collective behaviour is the best that the reviewer has yet read. It is concerned with analysing the conditions in which various types of collective behaviour or more accurately, perhaps, collective action, take place. It is well written and extremely well documented—indeed, some readers may find it over-carefully documented and consequently tending at times to become tiresome. This is, however, a relatively minor criticism. The author's analysis is made in terms of a 'value-added' model analogous to the 'value-added' model in economics, a concept which refers to the conversion of raw material into finished products. There is a number of stages in the process, and at each stage 'value' is 'added' to the final cost of the product. 'The key element', says Sr Smelser, 'is that the earlier stages must combine *according to a certain pattern* before the next stage can contribute its particular value to the finished product. ...Every stage in the value-added process, therefore, is a necessary condition for the appropriate and effective addition of value in the next stage'. As applied to collective behaviour, the concept means that a number of necessary conditions must be present, and as each condition combines with another, the type of resulting collective behaviour becomes more and more specifically defined.

Dr Rose's book is perhaps the most difficult of the three to assess, as is frequently the case with compilations of chapters written by different people. Despite the editor's efforts, the book inevitably lacks the kind of coherence one finds in Dr Smelser's and Dr Stark's books.

Human Behaviour and Social Processes is intended to familiarize readers with 'symbolic interaction theory' and work going on within its framework. The 'theory' itself, as outlined by Dr Rose in the opening chapter, is a form of role-theory, derived largely from Cooley and G. H. Mead, and stressing the continuous process of interaction rather than any more or less 'static' model of 'expectations' and 'role-requirements'. Dr Stark ought to be sympathetic to this kind of general point of view, and it is perhaps to be regretted that he did not deal with Mead in his own book. This reviewer is also sympathetic. The main difficulty seems to be the insufficiently precise relation between theoretical statements and empirical research and findings. Many of the separate chapters are extremely interesting and enlightening—for example, Robert Dubin's chapter on the 'central life-interests of industrial workers', Elwin Powell's on the 'beat generation' and Julius Roth's on 'the treatment of tuberculosis as a bargaining process' between patient and doctor. Perhaps the lack of a sense of systematic unity will be rectified in the near future by another and more systematic volume which could well be complementary to the present book. In the meantime, there are far too many items of interest in this book for it to be neglected, and it will provide quite a source-book for some years to come. It seems very doubtful, however, whether it can profitably be recommended as a text to any but fairly advanced students in psychology.

D. GRAHAM

Ego and Milieu: Theory and Practice of Environmental Therapy. By JOHN CUMMING and ELAINE CUMMING. New York: Atherton; London: Prentice Hall. 1962. Pp. xvi + 292. 60*s.*

At a time when the pharmacological approach to the treatment of schizophrenia is getting so much attention it is a relief to have a book which concerns itself essentially with the social environment of the patient. *Ego and Milieu* is written by two very experienced workers, a research psychiatrist and a sociologist who have long concerned themselves with the problem of the long-stay patient in mental hospitals. The authors have made considerable progress towards a systematic social psychological theory of mental illness and treatment. Their method is aimed at making maximum use of the social environment and differs from psychoanalytic theory in stressing the conflict free portion of the ego and the possibilities for personality change inherent in the structure of the environment. They borrow freely from previous work done in psychology and in sociology and introduce some concepts of their own about ego organization. The result is a book which stimulates a great deal of thought about current psychiatric practice in mental hospitals. It seems to the reviewer that it allows the psychologist, the social scientist and the psychiatrist a theoretical framework which embraces the thinking of all of these disciplines and offers a basically sociological approach to the modification of social behaviour. The therapeutic milieu they define as 'the scientific manipulation of the environment aimed at producing changes in the personality of the patient. Milieu is specifically built around the introduction of controlled crises in a protected situation.' The Cummings postulate that ego growth is essentially a series of disequilibriums and subsequent re-equilibriums between the person and the environment; therefore, by extension it seems reasonable that growth might be induced by presenting the individual with a series of graded crises under circumstances that maximize his chance of resolving them. In other words, crises resolution should be a therapeutic tool that can be scientifically controlled. Whatever one's views may be about the ultimate aetiology and treatment of schizophrenia it is difficult to see how anyone interested in the optimum use of the treatment environment can afford to ignore this important book. While it is difficult to read because of the very extensive field which it covers and the free use of sociological theory and ego psychology anyone who takes the trouble to study it will be richly rewarded.

MAXWELL JONES

Autonomous Group Functioning: and Exploration in Behaviour Theory and Measurement. By P. G. HERBST. London: Tavistock. 1962. Pp. xiv + 271. 36*s.*

In 1955 the D.S.I.R. and M.R.C. Joint Committee on Human Relations in Industry sponsored a study by the Tavistock Institute of the long wall system of coal mining. As is now well known, this and other subsequent studies by the Institute have led to the development by the Institute of the concept of the socio-technical system, whose detailed working out promises to provide new insights and a powerful new analytic tool for the study of industrial organization and behaviour.

At one stage in this research the opportunity arose to study 'a group of men who came together to test a new form of work organization, in which the team were given complete responsibility for the organization and performance of their task. The experiment lasted for three months. For this period a continuous day-by-day record became available of the activities and interactions of the group, which provided information about what each man did when, with whom, and for how long.'

Records were obtained and kept in great detail of many aspects of the performance and functioning of the group. The measures directly obtained were: man-shifts per work cycle, man-hours coal hewing, coal tonnage, work cycle time, time lost for external and for internal reasons, absenteeism per work cycle and hours of overtime. Herbst derives measures of the group processes involved and of performance satisfaction (assessed subjectively). It is the relationship between these measures and their connexions with the recorded events that form the core of the book. Emphasis is placed about equally on the methodological aspects and the substantive findings in this particular case study. As might be expected this results in an uneven treatment, passages of narrative of considerable interest alternating with scrupulous, at times pedantic, quasi-mathematical analysis.

As Herbst points out on p. 104: 'The findings suggest that the technical system does not determine the characteristics of the social system in any simple way. What appears to happen is that the social system that comes into existence adapts itself to the requirements of its task with a minimum change in its basic structure. At least in the present case the component structure appears to have been sufficiently viable to satisfy all task demands. Where a fit between social structure and task demands is not possible, some degree of conflict and impaired work efficiency may be expected to result.'

Herbst devotes a chapter to the mathematical theory that lies at the centre of his treatment and this can be recommended as a clear logical and honest statement of the mathematics, which is kept essentially simple. It is quite clear that the choice of parameters rests on the sociological and psychological insights and that they do not represent a unique set derived from the mathematical theory. This is a point of some consequence since it means that while the methods may be of general application, the derived laws governing the relationship of the observed phenomena need not be. However, the relationships predicted by the analysis are found to accord well with the observed data for this case study and to elucidate interactions among variables which would have been extremely difficult to argue without the mathematics.

The reader should be warned that this book does not make easy reading and the quest for rigorous definition can yield passages of this kind: 'The action potential is defined as an attribute of the action space, such that the greater the action potential, the greater the intensity with which the group seeks to move towards and maintain a specific action level which functions as a state of balance, and the greater the intensity of strain that arises to the extent that the pattern of functioning is disrupted. The action potential corresponds to an introjection-rejection process which operates so as to achieve and maintain a specific action level—that is, a specific set of activities and activity rates and a specific type of inter-relationship between activities—and to exclude other action levels. The operation of the introjection-rejection process thus results in the emergence of a functional boundary.'

At this point the reader's introjection–rejection process may be in the balance.

A. R. CHERNS

Contemporary Research in Personality. Edited by IRWIN G. SARASON. London: Van Nostrand. 1962. Pp. xii+411. *54s 6d.*

Sarason regards personality study '...as being primarily concerned with the isolation of variables relevant to the understanding, prediction and manipulation of individual differences and human variability'. Be this as it may, Sarason's view is that research work on personality as reported in the journals bears little relation to traditional theories of personality, and his aim is to provide the student '...with samples of empirical and theoretical issues, problems and methods characteristic of much current research in personality'. The contributions are necessarily diverse; they have been reprinted from a variety of sources, and the original dates of publication range from 1942 to 1961, with the majority in the last 10 years of this period. The publisher's note is in error in saying that there are 44 research papers; there are 40, and 66 authors. The various sections cover a wide range of topics including paper and pencil measures, projective methods, problems of assessment, development, social factors, perception, learning and stress, deviant behaviour and its treatment, and case studies.

The articles have been double-tested for interest and quality, once for publication in the journals, and once for inclusion in Sarason's collection, so the standard should be high, and on the whole it is. Most of the articles are short and condensed, and require fairly close attention; a few are unnecessarily complicated and abstract and less well expressed than they need be. In section II, item 4, the change expectancies in Table 1 appear to be in error, and Table 4 is poorly labelled and misleading. There is a fine example of a circular argument on p. 281. However, the book's merits far outweigh its defects; it can be recommended to advanced students of psychology, to clinical psychologists, and to anyone who wants to know what has been going on in personality study in recent years; it also provides an interesting test of one's general knowledge of psychology, and contains many supplementary references to previous work in particular areas.

D. BROMLEY

Roots of Behavior. Edited by EUGENE L. BLISS. New York: Harper and Row; London: Hamilton. 1962. Pp. xii + 339. $16.00.

This collection of twenty-three papers is based on a symposium held by the American Association for the Advancement of Science. It is concerned almost exclusively with animals and, as the title suggests, the dominating theme is the influence on behaviour of genetic factors and early experience. The book is divided into sections: six chapters on genetics, nine on instinctual behaviour, four on early experience and four on social behaviour. However, the labelling is for convenience only and many of the chapters refer to all these topics. The contributors are leaders in their subjects, and the symposium seems to have been planned with considerable thought. For instance, for several topics there is a chapter providing a general survey, but also one or more chapters giving detailed reports of individual experiments: J. P. Scott gives his general ideas on aggression in animals, and L. D. Clark follows with experimental studies on aggression in mice; J. L. Fuller and M. B. Waller ask the question 'Is Early Experience Different?', answer it in the affirmative, and the following chapters by S. Levine on infantile stimulation and by E. Hess on critical periods provide detailed evidence.

Several of the chapters have already appeared in other guise and it might be asked if yet another symposium is warranted, and at this price. In the event, the book is worth while for several reasons. It brings together most kinds of contribution being made in a subject of growing importance; the standard of work, argument and reporting is generally high; and, as compared with many so-called comparative studies, this one is truly comparative. There is emphasis on species differences and on individual differences within species, and in the few cases where reference is made to human behaviour (for instance in D. E. Davis's chapter on the phylogeny of gangs) the extrapolation is done with care.

Psychologists will find the chapters of genetics particularly useful. That by J. H. Bruell amounts to a primer in biometrics, and it is well and simply done. It shows how to set up an experiment in the genetics of behaviour and to analyse the results for dominance and segregation. In another chapter J. Hirsch summarizes his elegant experiments on the genetic basis for individual differences in *Drosophila*, and, in passing, provides important criticisms of those learning experiments which ignore such differences.

There are several chapters which explore the place of experiential and hormonal factors in instinctive behaviour. D. S. Lehrman discusses their interaction, and E. Shaw describes experiments in which she shows that, for the platyfish, general environmental visual stimulation rather than social stimulation is the important factor if normal sexual behaviour is to develop.

Of all the chapters, those on social behaviour fit least well into the pattern suggested by the book's title, but perhaps they can be regarded as a bonus.

There are some surprising omissions. For instance, there are many animals which are simple enough to allow correlations to be made between structure and behaviour. V. G. Dethier describes relevant experiments (including his own exemplary work on the blowfly), but it is disappointing in a book of this title to find no reference to the important series of studies on *Planaria*. In fact, the phylogenetic and ontogenetic roots have not been traced as far back as might be expected: there is nothing on protozoa and nothing on behaviour in embryo.

Most of the book is concerned with mammals. Among the remaining authors are W. C. Dilger, W. C. Young, H. F. Harlow, M. X. Zarrow, T. C. Schneirla, and C. R. Carpenter.

B. M. FOSS

Aggression: A Social Psychological Analysis. By LEONARD BERKOWITZ. New York and London: McGraw-Hill. 1962. Pp. xvi + 361. 62*s*.

This is a very useful book. The author has brought together a great deal of the experimental material on aggression, material which includes his own notable contributions to the subject. He presents a clear and consistent thesis which is, in fact, an elaboration of the 'frustration–aggression' hypothesis, put forward by Dollard *et al.* in 1939. A distinction, however, must be made between aggressiveness and aggressive behaviour. The latter may of course be an expression of aggressiveness, but it may be culturally expected conduct or purely instrumental, in which two cases it need not be accompanied by anger. The author is concerned with agressiveness and the aggressive behaviour to which it gives rise.

Aggressiveness, according to Berkowitz, always springs from frustration (though frustration does not always lead to aggressive conduct). The thesis is that frustration–aggression is, as it were, episodic. He does not envisage a continuous condition of anger. Anger is a response to a frustrating situation, and the first target is the frustrator. There is, however, a certain amount of generalizing, and anger may well be evoked by stimuli in some sense similar to the frustrator, particularly when the frustrator is inaccessible for one reason or another—such as fear of punishment. A great deal of frustrating, particularly if experienced by someone who has a low frustration tolerance, tends to give rise to an 'aggressive personality'; one, that is to say, who generalizes widely and views the world as a threatening place. Such a trait is likely to get established in a boy brought up by punitive rather than affectionate parents.

Furthermore, if overt aggressive behaviour of one kind or another is indulged in, a habit of aggressiveness gets established. For this reason the author is suspicious of all 'cathartic' theories according to which permitted aggressive conduct is supposed to 'drain away' pent-up anger. Since there is, according to Berkowitz, no anger 'pent up', this kind of permissive conduct merely reinforces the habit of overt aggressive behaviour. In addition, he produces evidence to show that, at any rate in the case of disturbed and highly aggressive children, violence witnessed on mass media has a deleterious effect.

The range of experimental material reviewed is remarkable, and goes to show the amount of ruthlessness which American social psychologists permit themselves. There is also a useful summary of the statistical evidence collected in connexion with aggressiveness among delinquent boys and girls. In the final chapter there is some interesting information about murderers and suicides.

W. J. H. SPROTT

Three Hundred Years of Psychiatry. By R. HUNTER and I. MACALPINE. London: Oxford University Press. 1962. Pp. xxvi+1107. Price £4. 4*s*.

This book consists of a series of short extracts from the works on insanity written by doctors, divines, schoolmasters and others between the years 1535 and 1860. There is also a brief account of each author and his place in British Psychiatry which forms a connecting narrative. The result is a fascinating and extremely interesting presentation of the progress of British Psychiatry from the Renaissance to the mid-nineteenth century. This excellent work is a welcome addition to the small group of books which deal with this rather neglected sector of the history of medicine. It should be read by all English-speaking psychiatrists and others who are interested in abnormal psychology. The book itself is extremely well produced and copiously illustrated.

FRANK FISH

Adventure in Psychiatry. By D. V. MARTIN. Oxford: Cassirer; London: Faber. 1962. Pp. x+207. 21*s*.

This book bears the subtitle 'Social Change in a Mental Hospital' and describes the radical changes which have taken place in the social structure of Claybury Hospital over the past six years. It is essential reading for everyone who works in a Psychiatric Hospital, especially those who have slipped into the way of thinking that they are doing the best that can be done under the circumstances. Claybury is an ordinary run-of-the-mill Psychiatric Hospital with no special resources, so that what has been done there could be done elsewhere by anyone with a social conscience.

Dr Martin and his colleagues have been able to prove decisively that many of the symptoms allegedly attributable to chronic mental illness are in fact due in large measure to the institutional treatment to which such patients are exposed. By opening up channels of communication at all levels, and by allowing patients to have a say in much of the running of the hospital, the whole atmosphere of the community has changed for the better, and therefore its therapeutic effectiveness.

The clinical psychologist or sociologist would be interested to know of the part played in this exciting experiment by the psychological members of the staff. They may well be surprised to read that the 'role of the psychologist in the therapeutic community is not yet clear. During a considerable part of the developments we had no psychologist on the staff, and at no time has there been more than one.' That a major experiment in social psychology should have been undertaken with no psychologist on the staff, even in an advisory capacity, is a sad reflexion on the ability of our profession to advertise its wares.

RALPH HETHERINGTON

Law and Psychiatry. By SHELDON GLUECK. London: Tavistock. 1963. Pp. ix+181. 38*s*.

Law and Psychiatry are alike in that they both deal with individuals who have transgressed beyond socially acceptable limits of human behaviour, but there the similarity ends. Lawyers take up an almost uncompromising attitude towards moral responsibility and freewill, without making explicit the basic tenets of their philosophy, whilst psychiatrists possess, at least implicitly, a deterministic approach to problems of behaviour. Differing in their approach and understanding, the two professions lack even a common form of communication, and most of the difficulties in court stem from the impossibility of explaining the concepts of one in the language of the other. Small wonder then, that lawyers deride the 'battle of the experts' and that at least 40% of them, according to a recent survey, fail to make use of the psychiatrist even when they believe this is indicated.

Into this area of conflict and misunderstanding Prof. Glueck brings his own extensive experience and expertise. Describing the dilemmas of the medico-legal partnership, as it exists both in court and in the mental hospital, he analyses the limitations of legal concepts and provides practical suggestions for resolving some of the many complex issues involved in determining responsibility. Apart from his own formulations for necessary reforms, the author also provides an integration of the most authoritative views on forensic psychiatry and criminal law. He examines the M'Naghten Rules and their American sequel, the Durham decision, and puts forward proposals for reform in sentencing and treatment.

In the past decade psychologists have played an increasingly active part in and behind the courtroom scene, taking over part of the function that traditionally belongs to psychiatry, and also its problems. Furthermore, courses in psychology are under consideration by those concerned with legal education, and if these are to be useful it is essential that university teachers are adequately orientated in this new field. This book, by one of the world's leading medicolegal scholars, can therefore be recommended to clinicians and academics alike.

L. R. C. HAWARD

The Short-Term Prisoner. A Study in Forensic Psychology. By R. G. ANDRY. London: Stevens. 1963. Pp. xxii+162. 28*s*.

Despite the logical place of the short-term sentence between the fine and longer periods of servitude, as a penal method it falls between two stools. It is not short enough to keep the prisoner free from the so-called evil and corrupting influences of prison life, nor is it long enough to enable adequate rehabilitation to be undertaken. As long ago as 1872 a Congress on Penology was asking for alternatives to short-term imprisonment, and in each of the ensuing decades penal reformers have pressed for a substantial change in the sentencing of courts. Yet comparatively recent surveys by Mannheim have shown that the proportion of short-term sentences had tripled during the last half century.

To find a rational and practical alternative, Dr Andry has conducted what he modestly calls a pilot study but which is in reality an investigation of some depth into 121 short-term prisoners. Despite the extreme difficulties of following up ex-prisoners of this sort, the author was able to obtain a great deal of factual information about these criminals; his conclusions and the findings on which they are based form the important part of this book. Statistical analysis, using a centroid factor matrix, produced high correlations between recidivism (as a criterion of failure) and emotional immaturity, juvenile crime experience, neuroticism, and extra-punitiveness. These are then used as the basis of a typological theory, the author's thesis being that the punishment should fit the criminal. While existing penal methods are seen as suitable for some of these recidivistic types, others need rehabilitative methods at present not available. The findings show up a serious deficiency in our prison system, which Andry proposes could be made good by means of Reconstruction Centres.

This is a most encouraging and stimulating book; despite the acknowledged limitations imposed by the sample size, it has established a pattern of inquiry which on a national scale promises to be of the utmost value to those concerned with penal reform. In shifting the emphasis from crime to criminal, and by underlining the need to classify offenders by some form of predictive typology, it provides a blueprint for further much-needed social and psychological investigations which could revolutionize the whole process of penal development.

On a wider perspective, this book is important in showing clearly the undeniable superiority of organized research over commissions of inquiry. A parallel investigation by the Home Office Advisory Council reached its conclusions on the basis of opinions expressed by official bodies called to give evidence: treating the short-term prisoners as a homogeneous group perpetuates the very problems reformers seek to solve, and to judge them on second-hand evidence continues a traditional weakness found in all such inquiries. In sharp contrast, Dr Andry's recommendations are positive, practical, and predictive, and it is to be hoped that these will have the impact they deserve.

L. R. C. HAWARD

Readings on The Exceptional Child: Research and Theory. Edited by E. PHILIP TRAPP and PHILIP HIMELSTEIN. New York: Appleton-Century-Crofts; London: Methuen. 1962. Pp. xii + 674. 55*s*.

This reviewer must confess to an idiosyncrasy on the basis of which he is liable to develop a violent allergic reaction to collections of papers bound up together to produce a *biblion abiblion* of 'Readings on...'. Past experience of symposia of one sort or another on the exceptional child—an awkward, portmanteau euphemism—had hardly stilled the apprehension with which he approached the present bulky volume. But, let it be said at once, this book is different. It is, indeed, an excellent book, if inevitably uneven. It is excellent, whether considered from the point of view of the teacher of psychology, of the professional neophyte, of the psychologist, psychiatrist, or social worker who is concerned as a matter of day-to-day educational or clinical routine, with problems of deviant, disturbed, or handicapped development in childhood.

The book consists of forty-eight papers, with brief introductory comments by the editors. Roughly one-third are published here for the first time, and most of the others have appeared in the journals in the past ten years, but the earliest is E. A. Doll's report as Chairman, 1938–39, of the Committee on Research of the American Association of Mental Deficiency. Several, like Kounin on 'rigidity' in the normal and the feeble-minded, or Terman and Oden on their gifted group at mid-life warrant the label of 'classic studies', although the critics of the classics are represented fairly (e.g. Spitz and Blackman; Stevenson and Zigler; Zigler). Some, like Eugene E. Doll's new historical survey of research and practice in mental deficiency in the U.S.A. are relatively massive and informative reviews of their field.

The articles brought together here are of two types, being 'specific experimental studies and critical reviews of a group of related experimental studies, pin-pointing basic issues or defining major trends'. Aetiological and psychometric studies are explicitly excluded. The papers fall into groups as follows:

Part I. Introduction—3 papers.
Part II. Exceptional Intellectual Processes—14 papers dealing with mental retardation and four with giftedness.
Part III. Exceptional Sensory and Motor Processes—five papers on the deaf and the blind; five on speech disorders, including aphasia; and eleven on brain damage and the physically handicapped.
Part IV. Exceptional Emotional Processes—six papers on schizophrenia in childhood, on autims, and other forms of emotional disturbance.

For those who are concerned in Great Britain with psychological research or advanced teaching in the areas covered by mental deficiency legislation or the special educational treatment regulations, this is unquestionably the most important and valuable single collection of primary source material now available. It may well serve to stimulate a new and more effectively tough-minded experimental approach to these problems, with advantage both to general psychological theory and to the applied psychology of these special fields. It is disappointing that a book that has been so well produced, and edited with so much intelligence and scholarship, should still reveal some obvious misprints (e.g. p. xii, *Schol Phobia*; p. 15, *Ira I9coe*).

GEORGE SETH

School leavers. Their Aspirations and Expectations. By THELMA VENESS. London: Methuen. 1962. Pp. xxvi + 252. 25*s*.

It was Morley who wrote, 'You cannot demonstrate an emotion or prove an aspiration'. Miss Veness, fortunately well aware of the second difficulty, kept it in mind in planning her research and in coming to her conclusions. Her researchers rightly attacked their problem from a number of directions, making enterprising use of free essays in conjunction with questionnaires, a Paired Comparison test, a Job Characteristic Attitude Survey, and an Interview. The results are headed, 'The Family', 'Work', 'Pre-occupations and motivational Patterns', with a chapter on 'Adolescence'. The findings are too numerous to be given in this short review.

The work has three faults. The first is that it attempts to cover so many aspects of aspiration, with so many groups of subjects, that the book inevitably becomes a long recital of differences between small groups, interesting at first but rather monotonous later, in spite of the considerable ability of the author in handling the material. It is, of course, much easier to be wise after the event than before, but concentration on far fewer aspects would have effected a big improvement. The other two faults interlock. First, the social class of the sample is unknown, owing to the refusal of the local authority to allow the question to be asked. Secondly, the sample is not representative. It follows that small differences between small groups might be misleading even though statistically significant. Though the deficiencies in the sample are fully acknowledged (on p. 23) and it would have been wearisome to have repeated qualifying phrases throughout the book, a more satisfactory solution might have been to relegate all small differences to tables in the Appendix, without tests of significance, and to have reminded readers now and again about the doubtful nature of the sample.

In spite of these criticisms the work is decidedly of value, particularly to research workers, and—in its own words—'affords tentative hypotheses for future research of a more rigorous nature.'

R. R. DALE

Social Welfare of the Aging: Edited by JEROME KAPLAN and GORDON J. ALDRIDGE. New York and London: Columbia University Press. 1962. Pp. xii + 372. $8.00.

Social and Psychological Aspects of Aging: Edited by CLARK TIBBITS and WILMA DONAHUE. New York and London: Columbia University Press. Pp. xviii + 952. $20.00.

The two volumes are from a series of four, which contain the papers given at the Fifth International Congress of Gerontology in San Francisco in 1960. Together they represent a general overview of the state of social and psychological research into aging in 1960.

The 40 papers in *Social Welfare of the Aging* deal with 'practical' problems of helping the aged, and are more likely to be relevant to the social worker than to the psychologist, though the psychologist may find incidental interest in the case study material and in the extent to which various countries seem to emphasize different welfare problems.

Approximately half the 100 papers in *Social and Psychological Aspects of Aging* are also likely to provide only background information for the psychologist, since they deal with the economic, social and cultural aspects of aging in many countries. Difficulties of generalization become all too clear in the face of the diverse modes of life and living preferences of the elderly found within many of the populations studied.

The sections on Mental Health, Personality and Leisure, contain papers on psychological topics such as reaction to institutional life, intellectual differences between people inside and outside institutions, emotional expression, and satisfaction with life. It is of interest that some attempts have been made to study attitudes towards death. This is indeed today's forbidden topic—in one instance, of 62 psychiatric geriatric patients interviewed, 54 said that they had never talked about death or dying to anyone before. A number of papers refer to 'successful aging', this concept being related to an inner state of satisfaction and happiness rather than to any one type of personality. Objective conditions must surely be taken into account here. For example, it was found that men entering an Italian home for the aged were maladjusted to the general problems of aging, while women mourned the fact that they had to be admitted to the institution. Another report showed that older people, both men and women, were much more emotionally disturbed than younger people by the sudden and total loss of their possessions, which they saw as irreplaceable symbols of their lives.

The experimental psychological papers touch on perception, intellectual decline on intelligence tests and problem solving tasks, rigidity, psychomotor learning, attitudes of the young towards the elderly, and longitudinal test studies of senescent twins. While all the papers are of interest, the data on the twins of 60 and over are particularly worth while, since they consisted of three sets of test results obtained over an eight-year period on the same subjects. (Decline in score between test sessions one and three was marked only on tapping speed, and digit symbol tests. Vocabulary, digit span, similarities and block design did not fall off.) Work which also seems of great potential value is described in three papers on the Halstead battery of neuropsychological tests. Originally designed to measure organic brain damage, there is some evidence to suggest that in a homogeneous population (with brain damaged cases removed or matched across age groups) the tests reflect differences roughly related to chronological age and more closely related to biological age.

It must be admitted that this reviewer is left with a feeling of dissatisfaction after reading these collections of papers. This stems not from defects of the individual papers so much as from the piecemeal nature of the research. For example, a body of knowledge about the psychological aging process is being built up, and all the time new findings confirm older discoveries. But it does not advance science much to show that yet one more psychological test score drops with age in a cross-sectional study. What does the drop mean in terms of physiology, or of everyday life, and would it be found anyway in longitudinal work? One might seek an answer to the first part of the question in volumes three and four of this series, those dealing with biological and medical aspects of aging, but here again there is rarely a link between the detailed findings of these disciplines and those of the social sciences.

However, there are a few attempts to get some inter-disciplinary results. There is the National Institute of Mental Health project, in which older subjects (47 men divided into 'healthy' and 'showing signs of ill-health') were compared with young college student volunteers on medical physiological and psychological tets. There is the Duke University Geriatric project, confined to people over 60, it is true, but longitudinal and already showing interesting results within that older age group. And there is a fine piece of cooperative research, reported by Burgess, in which seven investigators showed that while men at work displayed subtle physical signs of aging, it was rather the psychological and psychomotor changes which influenced the older men's self-perception of age. In order to carry out co-operative research, the research tools have to be ready, prepared by much previous individual research. Compared with that background labour, the initial inter-disciplinary results may seem small. But if the results of other one-discipline studies could be applied to similar inter-disciplinary efforts, and if these could preferably be longitudinal in character, research into the aging process might begin to move ahead very rapidly.

It goes without saying that anyone either interested or involved in work on social or psychological aspects of aging should read parts of these volumes. Undoubtedly they form a convenient collection of papers, many of which have been published since the conference, but prior to these volumes, in widely scattered journals.

SHEILA CHOWN

Scientists: Their Psychological World. By BERNICE T. EIDUSON. New York: Basic Books. 1962. Pp. xvi+299. $6.50.

Do research scientists, as a class, stand apart from others with regard to biographical background, emotional characteristics, cognitive style, inter-personal relations? This, in brief, is the important and difficult problem to which the author addresses herself. She selects forty male research scientists, more or less at random. Each is interviewed to elicit something of his biography and attitudes to various issues. He is also given a Rorschach and a TAT, and his records are handed over to three clinical psychologists who rate him on fifty variables. The interviews and the fifty ratings provide the data; and the bulk of the book is devoted to the author's discussion of her conclusions. The serious reader must regret that methodology is glossed over, that little is said about how the data generate the conclusions, and that such sparse attempt is made to deploy evidence with rigour or relate it to the work of others concerned with Eiduson's problem. This reviewer's main difficulty was to decide whether the book presents a careful empirical research, as the impressive appearance of the appendices suggests; or whether it presents a succession of impressions illustrated by excerpts from tape-recorded interviews. Erring on the side of caution, he decided that the latter was the case. Having decided this, he found the impressions interesting, persuasively expressed, and provocative of questions for research.

Those who are concerned with problems of occupational style and creative accomplishment will be stimulated by this book and enjoy its illustrative material. Those who want a reliable account of research findings will have to look elsewhere (a useful guide is McClelland's chapter in *Contemporary Approaches to Creative Thinking*). Those who work alongside scientists will hesitate to circulate this book as an example of how psychologists conduct and report their researches.

I. M. L. HUNTER

An Evaluation of the Bender-Gestalt Test. By A. TOLAR and H. SCHULBERG. Springfield, Ill: Thomas. 1962. Pp. xxiv + 229. $9.50.

The Bender–Gestalt Test is reported to be the third or fourth most frequently used test by clinical psychologists in the U.S.A., used at some time by 95 % of experienced clinicians. This survey of its practical use and theoretical contribution is the first of its scope since that of Benton, ten years ago. It, therefore, deserves a hearty welcome and careful study.

Dr Lauretta Bender herself contributes a 10-page foreword in which she briefly considers the test's theoretical and historical background, rationale, potentialities and limitations. It is clear that she, a neuropsychiatrist, does not approve of many of the uses to which it has been subjected by clinical psychologists. It is equally clear that they, in many cases, although willing to borrow the test operationally, do not find it possible to analyse its results or communicate about them in her way. This is a familiar situation in the clinical field.

The authors next provide a careful digest and evaluation of some 300 research papers. These are reported and discussed under the headings of (1) test figures and methods of administration, (2) perceptual and development factors, (3) personality and projective use, (4) psychoneurosis, including character and behaviour disorders, malingering and alcoholism, (5) psychosis, (6) organicity (*sic*), (7) Bender recall, (8) effects of physiological alterations, (9) intellectual functioning, and (10) methods of evaluating protocols. As well as summaries of each chapter, there is a final discussion of conclusions and implications. The authors comment on the test's 'deceptive simplicity', the lack of enhancement of the test's validity by the 'more objective approach' of the various scoring systems (one of which took fifteen hours per protocol), the 'great preponderance of low-level conceptualisation', and the lack of evidence for predictive validity as compared with concurrent. They also deplore the emphasis upon demonstrating small group differences, practically useless for individual diagnostic studies based on the test. They wonder whether the simplicity of the technique may not have 'prompted a deluge of ill-conceived research with relatively little concern for theoretical and methodological consideration'. The final note, however, expresses a cautious optimism.

This is a very brief comment on a book which already represents a considerable distillation of its source material. Clinical psychologists who use the test should certainly read it, and will profit from it.

J. A. L. NAUGHTON

Das Dorfspiel. By RUTH ZÜST. Bern and Stuttgart: Huber. 1963. Pp. 230. SFr./DM 34.

This book gives a practical account of the village technique of Henri Arthus as used in psychological work (diagnostic and therapeutic) in the school medical service of Zurich from 1949 to 1961. 174 photographs have been selected to illustrate points of method, development, form and content of Villages. The style of exposition is to give a brief general discussion of the topic in each chapter and then to devote most of the text to clarification by notes on the photographs. This is helpful, incidentally, to those of us for whom German is a foreign language. At the end of the five chapters there is a summary as well as a glossary of abbreviations and a very useful appendix with instructions for the test, and the eight standard questions to be asked when the village has been made, e.g. 'If this were a real village where would you like best to live?' (and 'alone, or with whom?'). The pieces are described (with measurements) and there is a footnote saying that they can be bought from the school materials department of Zurich for about 20 SFr. Earlier mention was made of the strong attraction of these brightly coloured materials in motivating children to begin at once to use them. Photography is recommended from experience as the most precise method of recording although Arthus had used sketching.

The Village method lays emphasis on the symbolic meaning of buildings, particularly the Church, the Castle and the Factory and Schoolhouse. The Church is the most important. It is associated with certain aspects of the mother figure, beyond those of the house itself which is the first life-space of the child and has specially close and warm associations with the mother, while at the same time representing the child himself, his 'Gestalt', and his means of social communion. Dr Züst ran a control series of villages with the co-operation of teachers in the Zurich school and found that only one child from 100 omitted the Church. The Castle brings out fantasies of law, might, authority—in short masculine identifications—in children of school age who have gone beyond fairy tales to history. The Factory and the School are less evocative of symbolic meaning, and are associated with work; deprived children are interested in factory products such as toys and sweets; choosing to live in the schoolhouse suggests some failure of the parent's home.

These examples of the relation of symbol and reality may seem dogmatic and *à priori*, but Dr Züst counteracts this with an implicitly normative approach, although norms are not formalized. She discusses villages made by the same individuals (not less than three from any child) from the points of view of development and movement in therapy. Formal aspects, i.e. the shape and the use of space, have a section to themselves, and the last section shows villages with signs of disturbance: regression as a defence; the expression of conflict; aggression.

The final part of the Appendix is headed—'Literature', and this is very meagre, consisting of one book and one paper by Arthus, a paper by Lombard-Rau and one by Mabille. The literature on this kind of projective technique is not large, but there is more than this. Dr Züst knows the work of Lowenfeld and Bühler but it is surprising that she seems not to know Mucchielli's book *Le Jeu du Monde et le Test du Village Imaginaire* (review in this *Journal*, **52**, 199–200) which would be of special interest to her because of the detail with which it traces the history of Arthus' Village Test.

Closer interaction of European and English-speaking psychologists is desirable, and it is to be hoped that this workmanlike publication will encourage clinics to introduce 'the Village' for comparative use with Lowenfeld's 'Worlds'. Its potential interest also for social psychologists is clear.

L. R. BOWYER

Statistical Principles in Experimental Design. By B. W. WINER. London and New York: McGraw Hill. 1962. Pp. x+672. 97*s*.

This book will be very useful to psychologists with a little statistical mathematics, and although it goes considerably beyond the scope of the ordinary text-book in elaborating mathematical principles and deductions, it does so only when these serve to elucidate research design. Statistical techniques are presented under the successive headings of general purpose, notation and computational procedures (expressed algebraically), followed by worked numerical examples, even for the most complex techniques. These make the book much more practical than most advanced texts. The reviewer found particularly interesting the accounts given of Type I and Type II errors; experiments involving repeated measures on the same subjects; nested factors in analysis of variance; geometrical diagrams of higher order interactions; and the clear descriptions of Hartley's and Cochran's tests for homogeneity of variance (with tables).

This volume is no lightweight; it is a 672-page McGraw-Hiller, only half of which is of immediate interest to research psychologists.

F. W. WARBURTON

The Experimental Psychology of Beauty. By C. W. VALENTINE. London: Methuen. 1962. Pp. xiii + 438. 45*s.*

Some apology seems due to Professor Valentine for the delayed appearance of this review. Truth to tell, this reviewer found the book rather heavy going—not because of any inherent difficulty in the subject-matter, nor, of course, because of any fault in the author's treatment. Indeed, Professor Valentine communicates his own enthusiasm most successfully, and one cannot but be impressed by the comprehensiveness of the survey he has made. The trouble probably lies in the nature of the work reported, much of which, particularly the early work in the visual field, was elementarist in its approach and pedestrian in its treatment. Later work has on the whole been more sophisticated and more stimulating. Further, although Professor Valentine has many cogent things to say on the difference between aesthetic experience and the *recognition* of beauty, one is left wondering how many of the researchers cited were fully aware of this distinction and the fundamental problems it raises. And still further—as one of these researchers, is one renegade in echoing Mr Somerset Maugham's *alter ego*, William Ashenden?—'Beauty is an ecstasy: it is as simple as hunger.... But people add other qualities to beauty—sublimity, human interest, tenderness, love—because beauty does not long content them.' Be that as it may, Professor Valentine's study is unique and long overdue, and as such earns our gratitude. B. SEMEONOFF

Skill in Sport: the Attainment of Proficiency. By B. KNAPP. London: Routledge and Kegan Paul. 1963. Pp. xii + 204. 30*s.*

International competition in the field of sport nowadays generally demands from budding aspirants to world titles a spirit of dedication to training and a mastery of the skills involved. The development of such an attitude of mind makes the difference between the top-flight athletic or sportsman and the 'run of the mill' performer. Miss Knapp has not addressed herself specifically to potential world champions in the various realms of sport and their coaches, and she has succeeded in persuading, even the 'rabbit', that ways and means can be found of acquiring skill—even to the extent of reducing one's handicap by a couple of strokes or of minimizing one's chances of ballooning the ball (the round version) over the bar when a yard from goal!

While the immediate appeal of this book is to the serious sportsman or sportswoman, their coaches and to specialists in the field of physical education, it should also be of interest to psychologists who are concerned in the study of skill. Individual differences, teaching methods, motivation, specialization are amongst the facets which are discussed against a background of experimental evidence and personal experience. A. MACDONALD

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A brief summary, suitable for use as an abstract, should precede the article.

2. Articles must be as short as adequate presentation allows. It is rarely possible to publish papers more than 7000 words in length, and most papers suitable for publication in the *Journal* gain by being much shorter than this. Supplementary material of special interest may be lodged with the Technical Information and Documentary Unit, Department of Scientific and Industrial Research.

It may be possible occasionally to give early publication to very short articles, not exceeding two printed pages in length (about 1000 words). As a rule such articles need not have a summary or titled headings.

3. The Editor cannot consider for publication articles seriously deficient in presentation. It is essential that a typescript submitted should represent the final form in which the author wishes the paper to appear. Authors may be charged for any proof changes (other than printers' errors) that may involve re-setting of lines or paragraphs. Insertion of new material involving re-paging, and changes in blocks made from diagrams or drawings, are so costly that authors are well advised to regard them as out of the question.

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RÉVÉSZ, G. (1951). Le problème du génie. *L'Année psychol.* **50**, 83–96.

Note that World List abbreviations are used for journals.

The Oxford Dictionary is taken as the authority on spelling.

Brit. J. Psychol. (1963), 54, 4, *pp.* 293–298
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PSYCHOLOGY, GENETICS AND THE PROCESS OF HISTORY*

By C. D. DARLINGTON
Botany School, University of Oxford

Almost everyone who writes about human beings, having described their behaviour, also offers to explain it. Biographers and historians, anthropologists and sociologists account for the properties of a man's mind by the character of his ancestors or the circumstances of his life, according to the way their inquiries happen to have led them; or by pure whim. This they do unembarassed by any acquaintance with psychology or genetics beyond the terminologies of those subjects. Nobody is surprised when the scholar in an account of the Greeks explains the clear ideas they, or some of them, had 2500 years ago by reference to the clear skies they had above their heads (Bowra, 1957).

Human behaviour has thus become a happy hunting ground for literary amateurs. And the reason is that psychology and genetics, whose business it is to explain behaviour, have failed to face their task together. This may seem odd when we recall that Francis Galton was in some sense the founder of both disciplines. But since his time the forces of inquiry have split into innumerable parties out of sight and sound of any common objective.

Within genetics itself we find ideas and methods which we can trace to Galton and to Mendel. And we find they have developed on different lines. Two conflicting systems of thought have grown up: the one values the synthesis of the whole heredity manifested in the individual; the other pursues the analysis of the parts of heredity which the process of inheritance entails. The conflict is never mentioned but, with consequences which we shall see, it has never been resolved.

In psychology the divergence and the conflict are more open but no more easily reconciled. Here there is a conflict which we can trace to Galton and Freud. To each of them we are indebted for enlarging our knowledge of the mind by developing one method and studying one side of its problems while disregarding the rest; disregarding, that is, what seemed to be not directly relevant at the time.

To begin with Freud (1931): he is quite candid. He tells us that he will not discuss heredity and he gives us two reasons. First, others have discussed it. Secondly we cannot change it. Both of these reasons were evidently derived from his reading of the German zoologist, Weismann. He forgot that in his youth he had observed that the 'common man' has a different psychology from ours. 'It would be easy' he writes (Freud, 1961) 'to demonstrate how "the people" judge, think, hope and work in a manner utterly different from ourselves. There is a psychology of the common man which differs considerably from ours. They have also more community spirit than we have; only for them is it natural that one man continues the life of the other, whereas for each of us the world comes to an end with our death.' (Vienna, 29 August 1883). Freud's professional work took him in a different direction. It took him far away from the common people. He began to attach more and more importance to external events at an early age, events he might hope to control, in explaining differences between the minds of individuals; and less and less importance to the internal character of the fertilized egg.

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We may say that since heredity works from the moment of fertilization Freud found himself (as Darwin had done in discussing twins) explaining it away by pushing back the effects of the environment to the beginning, to birth or gestation.

Galton's mistakes were quite different. They were not in fact his own but were the result rather of the startling success of his proposals in the hands of his successors. Let me deal with one of them. Galton said that twins were of two kinds: one-egg with the same heredity; two-egg with different heredity (Galton, 1876). We all know what extraordinary success this assumption has had as a means of comparing the effects of differences of heredity and differences of environment. Some investigators are so impressed by it that like Mendelian geneticists they forget that only *differences* are involved. They will tell you that 80% of the character of an individual may be due to heredity and 20% to the environment.

But this verbal error is a small matter compared with the basic assumption which is now treated as an axiom beyond discussion. Are one-egg twins bound to be the same in heredity? Are two-egg twins bound to be different?

With regard to one-egg twins, the basis of the assumption is that, since they arise from the same nucleus, they have the same outfits of chromosomes. But one-egg twins can differ: one may lose a chromosome, X or Y, and change its sex. Moreover the egg has to be split; it is itself asymmetrical in its cytoplasm; the process of its splitting and the results of the splitting are also asymmetrical, thereby demonstrating the effect of the inborn genetic character of the cytoplasm. These results may include the absence of a foot in one twin. But for us it is more important that they are likely to include a difference in temperament and intelligence (Darlington, 1954, 1963*a*).

We cannot blame Galton for not foreseeing these detailed consequences of the conversion of his assumption into an axiom of biology. No more indeed than we can blame him for not understanding the Mendelian paradox that heredity is what makes children differ from their parents as well as what makes them resemble their parents. But today we know these things and have to use them.

With regard to two-egg twins no cytoplasmic difference can exist: both are derived from whole eggs. The degree of their genetic differences depends solely on the chromosomes. It is a measure of the degree of outbreeding among their ancestors. But anything up to identity is possible. Thus one-egg twins can be genetically different. And two-egg twins can be genetically the same.

What follows? It is that we have to admit that comparisons of one-egg and two-egg twins do not give us the uncontaminated separation of heredity and environment which Galton and his successors have hoped for. The measure they give us is an estimate: but it must always be an *underestimate* of the effects of heredity.

Similar objections and qualifications may be raised with regard to each separate approach to genetic psychology. It is only when we combine all approaches that a convincing total picture is obtained. The study of twins (Kallmann, 1959), and of foster-children (Lewis, 1960), the use of educational experience (Burt, 1961), the following of both hereditary genius (Galton, 1869) and of criminal pedigrees from the Jukes to the Mafia (Dugdale, 1875; Sondern, 1958), the subdivision of mental deficiency (Roberts, 1952) and the treatment of illness (Kallmann, 1959), the assessment of animal breeding for temperament and intelligence (Fuller & Thompson, 1960; Roe & Simpson, 1958), the interpretation of chromosome balance and mutation, when these are taken together with contemporary psychology and genetics (Darlington, 1963*a*) sloppy thinking and loose talk can gradually be excluded.

Take the problem of adoption. When we compare the professions of adopted children with those of their foster parents, who are often of better social class than their natural parents, we find a disparity. The adopters being more numerous in higher social classes than the adopted, the child is often *over-placed.* Not all the efforts of societies desperately seeking the happy upbringing of their charges, nor all the volumes written to explain the results, can wipe out that little word. A social class is already imprinted on the newborn child: it is imprinted by heredity and it is therefore a genetic class.

From what is compatible with all the diverse kinds of inquiry a genetic psychology begins to emerge. It then becomes possible and necessary to face larger issues. The largest issue is that of understanding the general process of history. We cannot afford to neglect this gigantic process because it has been by its means, especially by the evolution of society, that man, or rather particular men, have created their and our own environment. And it is this environment which constitutes one half of the heredity-environment polarity on which all psychological and genetical discussion turns. It so happens that we have recently been given the means of studying the process of history by the technical advance in dated archaeology.

From the point of view of psychology and genetics history has four stages. The first stage lasted in its full expanse until 10,000 years ago (Coon, 1962). At that time mankind consisted of some ten million people distributed over most of the earth in thousands of breeding groups usually known as tribes, a few of which, very few, still survive, almost as they were then.

These tribes had (and their survivors still have) two remarkable properties. In the first place they were all different from one another and aware of their differences. They were all, it would seem, adapted to their climates, habitats, and ways of life. They were adapted by physique, temperament and intelligence. They were adapted to making their living by hunting, or fishing, digging or collecting. And that they were adapted genetically we know as well from Darwin as from the most recent studies of haemoglobin (Darlington, 1958).

In the second place each group or tribe was uniform in this basic adaptation. Each individual was capable of doing everything necessary to maintain life. The only differentiation of activity or capacity depended on the one genetic differentiation of sex. And what they could and can do in this matter of maintaining life was and is often far beyond the capacity of civilized men and women.

The second stage of history, which lasted some 4000 years or 120 generations, was marked by the origin of new people with new capacities. It happened in S.W. Asia. It depended on the domestication of plants and animals. And the new people were the races of men responsible for this revolution.

How did it happen? It happened by a process discovered by Darwin with implications far beyond what he was imagining. The process was one of what he called unconscious selection. Man propagated crops and stock. He unconsciously selected them. They changed out of all recognition. But as they improved and multiplied he came to depend on them for his survival. Those men who did not improve in their skill were eliminated. The crops and the stock were now selecting Man. He was being selected for new qualities of mind and body. Patience, industry and foresight of new and diverse kinds were selectively at a premium. A sense of property was added to a sense of territory. Understanding and love of plants, animals and the soil were divergently developed in new races of peasants and herdsmen (Raglan, 1939; Darlington, 1963*c*).

A vast amount of thought and observation has been devoted to the study of differences between the new kinds of people and the old kinds of people. The results cannot be of indifference to the psychologist or the geneticist. In our own country, take the gypsy, incapable of settling, loving and knowing horses, with his special arts of snaring, and tinkering, basketry and music, arts common to his race all over the world; moreover, somewhat unconscious of property and cleanliness. Compare him with the grain cultivator, refusing to budge, knowing and loving the soil and the place of his birth, with his arts all special to his locality or rather to his breeding group; highly conscious of property and often of cleanliness. Which is more intelligent? That we cannot say, for a single scale of measurement becomes meaningless. All we can say is that their gifts, which are extremely different, are obviously adaptive and are recombined or destroyed by interbreeding. They are therefore genetic (Darlington, 1959).

The third stage of history reached its climax in the next 2000 years. In this period different kinds of people came together. Different tribes combined without freely interbreeding. They combined because their different gifts were mutually useful. They formed societies. But these societies were composed of classes or castes which retained their character because they continued to breed within the caste or class almost as strictly as they had within the tribe (Darlington, 1963*c*).

It happens just the same today. Different kinds of people do not usually want to mix and eat together. It does not surprise us that the discriminating Indian peasant should object to mixing with the carrion-eating hill-tribesman. What is more remarkable is that the same prejudice, the same psychological principle, operates among Christians. It operates even between men of the same nation and colour: our own nation and our own colour. It was only in the last century that an Oxford don remarked *à propos* of broadening the admission to his college: 'few of us, after all, have ever seen a dissenter eat'.

Co-operation without cohabitation is thus the invariable process by which all stratified societies, that is to say all advanced societies, have come into being. By it the separate tribes, freely spread like an extended pavement, are now piled up into the towering masses of our social classes.

Today, 4000 years later, we find ourselves in the fourth stage of human history. The original differences between races in the stratified societies have partly disappeared by slow inbreeding. Sometimes also the class structure itself has almost disappeared. But as a rule it has survived and elaborated itself. With it the differences in intelligence and temperament which adapt each class to its different functions in society have been preserved by constant selection, and above all by selective mobility, up and down in society (Burt, 1961; Darlington, 1963*c*). The overriding principle, which no society can evade, is that the character of each class is maintained by the ability of its members, genetic and psychological, bodily and mental, to make its living by its own kind of work.

Put these views now in relation to the ideas of our predecessors. Galton and Freud both knew that differences between individuals and classes were due to heredity. How these differences have arisen and maintained themselves we can now understand in terms of Darwin and Mendel. But, further, we can see that, when different kinds of men came together to produce diverse social classes, for the first time in history diverse environments were brought together within one society. We can see what this means by a familiar example. We see different people sitting side by side in the same railway compartment. But they are living in different environments, for on the one hand they are reading different papers; and on the other hand they do not intend, or expect, or desire, to speak to one another. In urban societies, our lives are largely made up of such situations. They may be summed up by saying that we are engaged in choosing, and being chosen by, environments that fit us in body and mind.

If most human beings, however, appear to fit their environments it is not on account of any active effort. Most people are living in environments that have been created by or for their own kind of people with their own kind of heredity: people of their own class. Effort and choice have been necessary only if the environments were different, either higher or lower than, those their parents or foster parents gave them. The environment of man is thus a diversity of environments. And it has been created by a diversity of heredities.

A few people do not fit any environment that their society can offer them. These we may say include the genius who, rejecting what is offered him, creates a new environment for himself and perhaps for others. They also include the delinquent who, likewise rejecting what is offered him, finds it easier to destroy than to create.

These views are not simple although I have simplified them. But I suggest that they may help psychology and genetics to cut a way out of the verbal briars and brambles in which they have recently (and separately) entangled themselves. So they might be able to play the part they should play, a central part, in understanding mankind and society.

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NEGATIVES: DENOTATION AND CONNOTATION

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The hypothesis was investigated that the connotations of the word 'not' affect the times taken to verify negative statements more than the function of this word as a logical constant. The task was to verify statements which asserted, or denied, that a given number was 'even'. Two groups were used: an explicit group in which the statements were in English, and an implicit group in which neutral signs stood for assertion and denial, their functions being learned by discovering the conditions which made the statements true and false. The results showed that the difference between the response times to explicit affirmative and negative statements was significantly greater ($P < 0{\cdot}001$) after practice than the corresponding difference for implicit statements. The introspections suggested, however, that the subjects' approach to the implicit statements was an important variable. Tentative evidence for two factors affecting the response times to negative statements was adduced.

I. Introduction

Previous experiments (Wason, 1959, 1961) have shown that negative statements are responded to more slowly than affirmative statements, even after practice and when the amount of information conveyed by both kinds of statement is equated. The present experiment is designed to test the hypothesis that the connotations of the word 'not' affect the times taken to verify negative statements more than the function of 'not' as the logical constant of denial. A distinction is drawn between (*a*) the function, or denotation, of the word 'not' in declarative contexts, i.e. the fact that it reverses the truth values of an affirmative statement, and (*b*) the possible associated meanings, or connotations, of this word. Hence the *explicitness* of the statements is varied, in addition to their *polarity* (affirmative or negative) and *truth value* (true or false). An *explicit* statement is defined here as one which contains the words 'is' or 'is not'; an *implicit* statement as one in which these words are replaced by neutral signs (nonsense syllables). The functions of these two syllables are learned by discovering the conditions which make the implicit statements true and false. It follows that the syllables do all that the words 'is' and 'is not' do in explicit statements: one syllable asserts a statement and the other denies it. Hence the implicit statements acquire the same verifiable consequences as their corresponding explicit statements; i.e. they acquire the same 'cognitive meaning', or denotation. The syllables, however, differ from their corresponding words because the meaning which they acquire is restricted to the statements in which they are used, whereas the words already possess meaning from their use in other contexts.

The hypothesis allows the prediction that the difference between the times taken to verify explicit affirmative and negative statements is greater than the corresponding difference for implicit statements. In a related experiment, Eifermann (personal communication) found that, when the affirmative and negative attributes of statements were replaced by abstract signs, the response times to negatives tended to be reduced. Her subjects, however, learned the verbal meaning of these signs before starting the task. In the present experiment the verbal meaning of the assertion and denial syllables was withheld in order to minimize the possible effects of verbal connotations.

II. METHOD

Design

The material consisted of statements which either asserted, or denied, that a given number is 'even', the task being to determine whether they were true or false. Forty-eight subjects (Psychology undergraduates) were selected at random and allocated alternately to the *explicit group* (statements in English) or the *implicit group* (statements containing nonsense syllables). After initial practice, a timed series of forty-eight statements was presented, arranged in accordance with the permutations of the serial order used previously (Wason, 1959). One example of the four types of statement (true affirmative, false affirmative, true negative, false negative) occurred once in every block of four trials, making a total of twelve presentations for each type.

Procedure

Subjects were tested individually. A practice series consisting of a list of twenty-four statements (six examples of each of the four types arranged at random) was first presented. The first two statements in the list, a true and a false negative, were marked with a tick and a cross respectively by the experimenter. The task was to tick or cross each successive statement, according to whether they were judged true or false. The experimenter told the subject the correct response each time.

In the implicit group two nonsense syllables, *Dax* and *Med*, were chosen as assertion and denial signs. For half the subjects *Dax* functioned as the assertion and *Med* as the denial sign; for the other half these functions were reversed. For example, the first subject had *Dax* as the assertion and *Med* as the denial sign, the first four statements in the list, with the correct responses, being:

'7 MED an even number': ✓
'4 MED an even number': ×
'8 DAX an even number': ✓
'3 DAX an even number': ×

In the explicit group the series was identical, except that the words *is* and *not* replaced the assertion and denial syllables. In the negative statements the word *is* was omitted in order to control the length of the statements. The reasons for doing this were explained to the subjects informally.

The instructions to the implicit group were: 'All I want you to do first of all is to find out whether these statements (which contain one of two nonsense syllables) are right or wrong. You will see that the first statement is right and the second one wrong. I shall tell you whether your answer is correct, or not, when you put a tick or a cross against each statement. You will not be timed. Have you any questions?' (The instructions to the explicit group were the same except that reference to nonsense syllables was omitted.)

After the practice series subjects were seated in front of an apparatus in which the statements had been arranged in accordance with the appropriate permutation on a moving belt. By turning a handle the experimenter could align each statement in turn behind an aperture, and illuminate it by pressing a button which simultaneously started a 'Dekatron' automatic timer. The subject responded by pressing a key marked with a tick, or with a cross, which stopped the action of the timer and switched off the light behind the statement. Two additional pilot lights behind the apparatus informed the experimenter which key had been pressed, the subjects being told whether their response was correct, or not, each time.

The instructions were: 'Now we continue with the same sort of thing on this machine. You see that one key has a tick (which means right), and the other a cross (which means wrong). You will be shown a series of statements like the ones you have been doing, and have to press the appropriate key in response. You will be timed, but this is because we are interested in the time you take to get the answers correct: accuracy is more important than speed. If you respond incorrectly, we may have to keep on doing them until all your answers are correct. Have you any questions?'

III. RESULTS

In the implicit group all the subjects rapidly learned the correct responses in the initial practice series. Figs. 1 and 2 show the mean response times in seconds to the four types of statement, each presented twelve times, for the explicit and implicit groups respectively. It will be noted that the curves of the four types of statement are much more clearly distinguished when they are explicit, compared with implicit. Table 1 shows the number of errors made on the first six presentations of the task, compared with the second six, for both groups. On the second half of the task it is evident that there is little difference between the number of errors made in the two groups, but on the first half far more errors occur in the implicit group. Hence, since learning would appear to be effectively completed by the seventh presentation of the statements, a three-way analysis of variance (Edwards, 1951), modified by Jonckheere (personal communication), was computed on the sum of the response times from the seventh to the twelfth presentations, the individual observations having been first converted into reciprocals, which ensured homogeneity of variance. Table 2 shows the response times, re-converted into harmonic means (reciprocal of mean reciprocal) for ease of reference. Of the main effects, both polarity and truth value are significant ($P < 0{\cdot}001$); i.e., in general, response to affirmative statements is faster than to negative ones, and to true statements faster than to false ones. The effect of explicitness is not significant. The interactions between (*a*) polarity and explicitness, and (*b*) polarity and truth value are both significant ($P < 0{\cdot}001$). Inspection of Table 2 shows that the difference in response times between affirmative and negative statements is much more pronounced when the statements are (*a*) explicit, as opposed to implicit, and (*b*) true, as opposed to false. The interaction between polarity, explicitness and truth value is also significant ($P < 0{\cdot}05$). It will be noted that the interaction between polarity and explicitness is pronounced when the statements are true, but minimal when false. Fig. 3 shows the trend of this interaction over all twelve presentations for true statements alone. It will be noted that, when the statements are negative, response to implicit ones becomes faster than to explicit ones from the third presentation onwards, but, when the statements are affirmative, response to implicit ones only becomes faster at the final presentation.

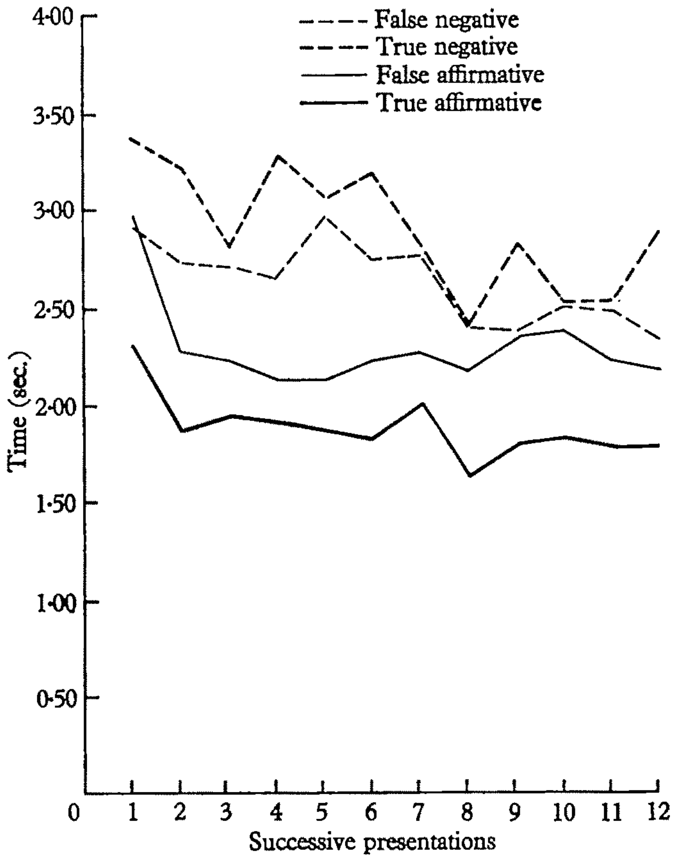

Fig. 1. Mean response times to explicit statements.

The introspections gave information about the frequency with which the subjects attached verbal meaning to the implicit statements, and also about the strategies adopted to facilitate the responses. A third of the subjects in the implicit group reported 'interpreting' the nonsense syllables in terms of their linguistic equivalents, e.g. 'right', 'is', 'true', 'the same', 'not true', 'no', 'isn't', 'reverse', etc. The majority of subjects in this group, however, reported a determination of the responses on the basis of the formal characteristics of the statements—an approach reported by only three subjects in the explicit group. These 'decision procedures' provided a formula which classified true statements as equivalent regardless of whether they were affirmative or negative, e.g. 'if an even number and Dax, or an odd number and Med, then press "tick"; otherwise press "cross".' Three subjects in the implicit group reported using both of these approaches.

Table 3 shows the number of subjects who reported these approaches to the task, together with the harmonic mean response times associated with them from the 7th to the 12th presentations of the statements. Only true statements are considered, however, because of the interaction between polarity and truth value, which tends to obscure the effects of the negative. The four subjects in the implicit group who did not report using either approach are included on the assumption that they were, in fact, responding to the formal characteristics of the statements. This assumption is supported by inspection of their response times. It will be noted that the difference between the affirmative and negative latencies is least when implicit statements are

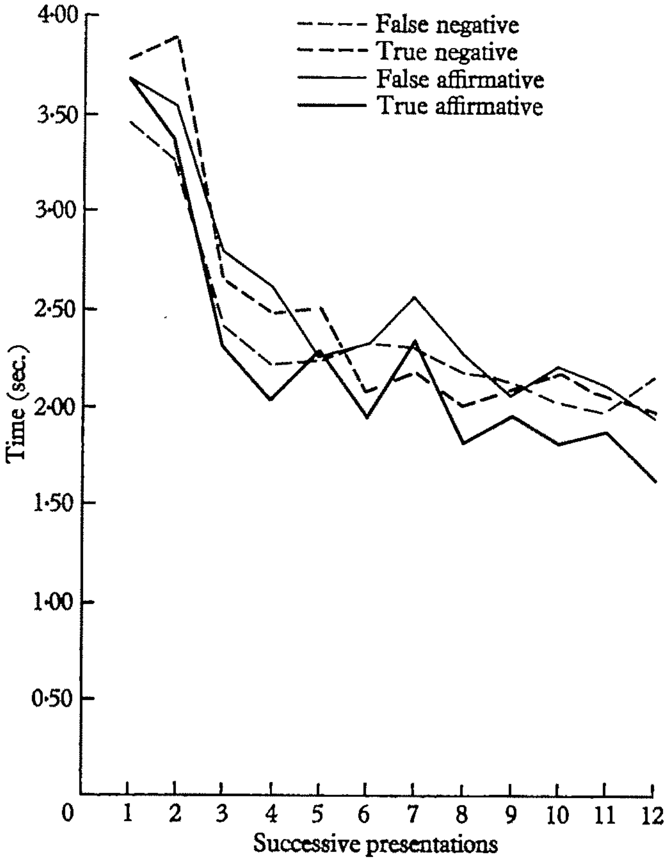

Fig. 2. Mean response times to implicit statements.

Table 1. *Frequency of errors* (*cell maximum* = 144)

Presentations ...	1–6		7–12		
Group ...	Explicit	Implicit	Explicit	Implicit	Total
True affirmative	3	15	1	6	25
False affirmative	5	12	2	2	21
True negative	6	21	8	7	42
False negative	5	10	6	4	25
Total	19	58	17	19	113

not interpreted (0·14 sec., N.S., $n = 16$), intermediate when such statements are interpreted (0·42 sec., $P < 0{\cdot}05$, $n = 8$), and greatest when the statements are explicit (0·71 sec., $P < 0{\cdot}0001$, $n = 24$) (related mean t-tests). In addition, the

Table 2. *Mean response times in seconds (7th-12th presentations)*

	Affirmative	Negative
Explicit	1·86	2·29
Implicit	1·79	1·87
Total	1·82	2·06

Polarity, $P < 0{\cdot}001$

Polarity × explicitness, $P < 0{\cdot}001$

	True	False
Affirmative	1·67	2·00
Negative	2·10	2·02
Total	1·86	2·01

Truth value, $P < 0{\cdot}001$

Truth value × polarity, $P < 0{\cdot}001$

	Explicit	Implicit
True	1·95	1·77
False	2·14	1·89
Total	2·04	1·83

Explicitness, N.S.

Explicitness × truth value, N.S

	True		False	
	Explicit	Implicit	Explicit	Implicit
Affirmative	1·66	1·68	2·08	1·92
Negative	2·37	1·89	2·21	1·86

Polarity × explicitness × truth value, $P < 0{\cdot}05$.

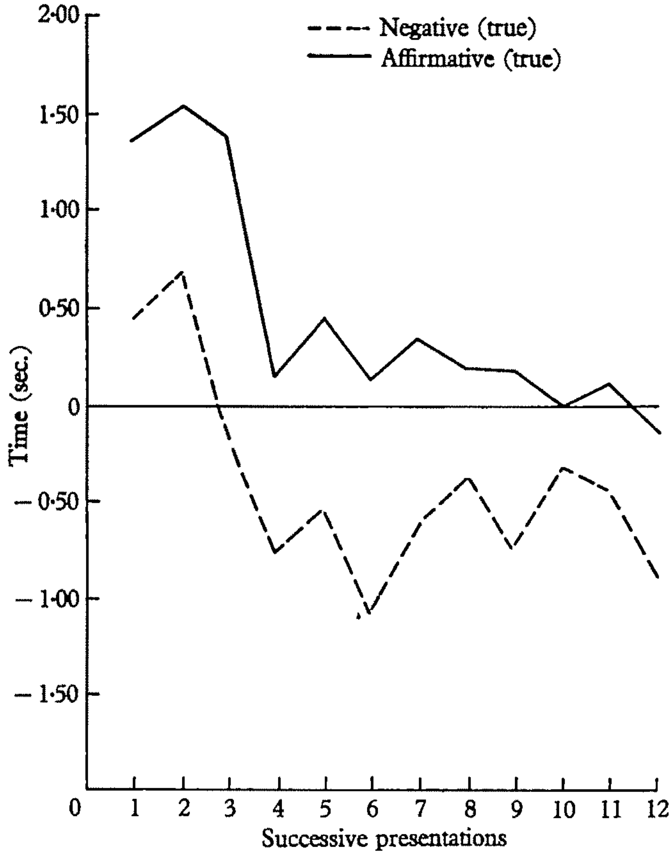

Fig. 3. Difference between mean response times to implicit and explicit statements (implicit minus explicit response times).

magnitude of the difference associated with explicit statements differed significantly ($P < 0{\cdot}01$) from the corresponding difference in interpreted implicit statements, but the latter difference did not differ significantly from that in uninterpreted implicit statements.

Comparing the response times to explicit statements with those to interpreted implicit statements, it will be noted that the latencies to the negatives are very similar in magnitude. However, when implicit statements are interpreted, the latency to affirmatives is considerably greater than the corresponding value for explicit statements. The additional process of interpretation would be expected to increase the over-all response times to implicit statements, but the fact that the negative latency is not correspondingly increased is presumably a function of the implicitness of the denial nonsense syllable.

Table 3. *Mean response times in seconds (7th–12th presentations) to true affirmative and negative statements as a function of the approach to the task*

		N	Affirmative	Negative	Difference
Implicit statements, not interpreted	Decision procedures	12	1·56	1·69	0·13
	No decision procedures	4	1·63	1·82	0·19
	Total	16	1·58	1·72	0·14, N.S.
Implicit statements, interpreted	Decision procedures	3	2·05	2·46	0·41
	No decision procedures	5	1·84	2·26	0·42
	Total	8	1·91	2·33	0·42, $P < 0{\cdot}05$
Explicit statements	Decision procedures	3	1·65	2·47	0·82
	No decision procedures	21	1·66	2·36	0·70
	Total	24	1·66	2·37	0·71, $P < 0{\cdot}0001$

IV. DISCUSSION

The significant interaction between polarity and explicitness ($P < 0{\cdot}001$) confirms the prediction that the connotations of the word 'not' affect the response latencies to negative statements more than the function of 'not' as a logical constant. The mean response times, over the second half of the task, to explicit statements is 1·85 sec. for affirmatives and 2·29 sec. for negatives, a difference of 0·44 sec. The corresponding values for implicit statements are 1·79 and 1·87 sec., a difference of 0·08 sec. When only true statements are considered the interaction is even more pronounced, the differences being 0·71 sec. for explicit and 0·21 sec. for implicit statements.

An important difference between the groups, however, in their approach to the task, is the much greater frequency with which decision procedures were reported in the implicit group. These procedures, based on the formal characteristics of the statements, are analogous to computer programmes in so far as they enable the assertion and denial syllables to be treated as having equal and independent functions with respect to the response. Hence their adoption would seem to be the most efficient way of tackling the task. But it will be noted from Table 3 that the deliberate use of decision procedures tends to reduce the difference between the affirmative and negative response latencies only when implicit statements are not given a verbal interpretation. When such statements are interpreted, and when the statements are explicit, the effect of such procedures appears to be either negligible or detrimental. Thus, although decision procedures clearly play a predominant role in confirming the prediction, owing to their frequency of occurrence in the implicit group, they have little or no effect on the few cases in which the statements were also construed in a meaningful way.

A more illuminating result (see Table 3) is that the differences between the response latencies to affirmative and negative statements vary as a direct function of the extent to which the statements possess, or acquire, verbal meaning: 0·71 sec. ($P < 0{\cdot}0001$, $n = 24$) for explicit statements, 0·42 sec. ($P < 0{\cdot}05$, $n = 8$) for interpreted implicit statements, and 0·14 sec. (N.S., $n = 16$) for uninterpreted implicit statements. These figures must obviously be accepted with caution as they suffer from the limitations of introspective evidence, and from the small frequencies in some cases, but they do strongly corroborate the hypothesis. More specifically, they suggest (*a*) that the logical function of denial, exercised by an uninterpreted nonsense syllable, creates little difference between the response latencies to affirmative and negative statements, (*b*) that verbal interpretation of the negative does produce a significant difference between these latencies, and (*c*) that the specific effect of the stimulus, 'not', produces an additional difference.

Possible connotations which could affect the response latencies to negative statements have been discussed previously (Wason, 1959, 1961). The most obvious one is connected with the fact that, whereas assertions are generally used to give information about a state of affairs, denials are used to remove misconceptions about it. This subsidiary, or modifying role which denials play in language is assumed to be associated with negative statements in general (particularly when such statements occur in isolation), and tends to result in a compulsion to translate them into affirmative form before evaluating them. Some evidence for the operation of this process was obtained in a previous verification experiment (Wason, 1961). This tendency to translate negative information is not, of course, specific to the present type of task. It has been studied in relation to problem solving and conceptual thinking by Bruner *et al.* (1956) and by Donaldson (1959), among others, but in these contexts translation of negative information is more appropriate since the information has to be used for reaching a solution, as opposed to being judged simply true or false.

What is more difficult to explain on the assumption of the 'translation hypothesis' is that the difference between the affirmative and negative latencies to explicit statements (0·71 sec.) differs significantly ($P < 0{\cdot}01$) from the corresponding difference in interpreted implicit statements (0·42 sec.). In both cases the negative statements are evidently interpreted as denials, but in the former case this meaning is mediated directly by the word 'not', whereas in the latter the subjects are free to select their own covert verbal response. Hence it is possible that the difference is due to the affective connotations of the stimulus, 'not', (derived from negative imperatives) which may inhibit the response to explicit negative statements.

Eifermann (1961) provides some evidence for this specific stimulus effect. In tasks similar to the present one, she experimented with two kinds of negative term which exist in the Hebrew language, one of which is used in all contexts including the prohibitive, and the other in all contexts except the prohibitive. Whichever term was used there was a significant difference ($P < 0{\cdot}001$) between the response times to affirmative and negative statements. However, the response latencies to statements containing the negative normally used in all contexts were significantly greater ($P < 0{\cdot}05$) than those to statements containing the negative normally used in all contexts except the prohibitive. This result is convincing because, even disregarding its use in prohibitive contexts, the frequency of usage of the former term is greater than that of the latter.

The present results suggest that two factors may account for the differences between the response latencies to affirmative and negative statements: (*a*) the assumed tendency to translate negative statements into an affirmative form because of the role which denials play in language, and (*b*) an inhibition of response specifically associated with the prohibitive connotations of the word 'not'. Further light might be thrown on the effects of the latter factor by deliberately varying the level at which the statements are interpreted. The effects of three experimental conditions can be distinguished: (*a*) that of the stimulus word 'not'; (*b*) that of a denial sign the meaning ('not') of which is known to the subjects beforehand (stimulus varied, interpretation constant); and (*c*) that of a denial sign the meaning of which the subjects are left to infer (both stimulus and interpretation varied). It would, however, be desirable to minimize the use of decision procedures, as a means of determining the responses in the implicit task. This could be done by using neutral signs for the negative statements only, leaving the affirmative statements explicit in form. It would then be more difficult and unrewarding to develop decision procedures to cover the responses to both types of statement, and the tendency to interpret the denial syllable would consequently be maximized. Using this modified task it might be possible to discriminate between the effects of the assumed prohibitive connotations attached to 'not' (whether induced explicitly or implicitly) and the effects of substituting a neutral sign for this word.

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VIGILANCE CONSIDERED AS A STATISTICAL DECISION

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A number of experiments are described in which untrained subjects kept watch during long periods for inconspicuous signals. When they reported such a signal, they had to indicate their degree of confidence that the report was correct. By analysing the accuracy of reports in each category of confidence, it can be shown that they are quite inconsistent with the concept that stimuli are either definitely perceived or definitely fail to be detected, with no intermediate category. The results are more consistent with a model of perception as the outcome of a statistical decision made with more or less caution upon evidence contaminated by randomness.

Analysing the empirical data according to this model, neither the duration of a watch nor the presence of loud noise during visual experiments affect the degree of randomness in the evidence used for decision. Both factors do, however, affect the degree of caution with which the decision is made, and this may explain the failures of perception previously found in experiments upon prolonged watch and upon noise. Changes during the watch appear primarily, however, when the decision is being made cautiously (with few false detections) while the changes with noise appear rather as a lessening of the difference between cautious and risky behaviour. This divergence between the effects of noise and of prolonged work may explain the fact that experimental conditions which reveal effects of noise have not always in the past been those which are sensitive to effects of prolonged work.

I. INTRODUCTION

For nearly 20 years psychologists have been much concerned with the rapid decline in efficiency which men sometimes show when keeping watch for inconspicuous signals. Such signals may be, for instance, echoes on radar screens or minor flaws in manufactured products: one recent experiment studied the detection of satellites by visual means, and found that observers did much worse after only half an hour of watching (Garvey, Gulledge & Henson, 1958). Such an early deterioration occurring in the absence of physical work, might be taken as evidence for some inhibitory state within the central nervous system, and is certainly of considerable theoretical interest. A number of investigations have been surveyed by Broadbent (1958); the main explanations distinguished by him and by other reviewers such as Frankmann & Adams (1960) and McGrath, Harabedian & Buckner (1959) are in terms of expectancy, arousal and distraction.

The first of these theories holds that the threshold for any signal, below which it will not be detected, rises when the probability of that signal is low. When a man watches for the signal for a long time without its occurring, detection becomes poor simply because his estimate of signal probability becomes low.

The second theory holds that detection is more or less likely depending upon the general level of 'arousal' of the nervous system, which in turn depends upon the time of day, the amount of recent sleep, the amount of varied stimulation reaching the senses, and the level of motivation or incentive. These assumptions about arousal are physiologically plausible (Samuels, 1959). A prolonged period of watch allows little variety of stimulation and therefore causes a drop in the level of arousal.

The third theory argues that the information reaching the human senses is in any case filtered, so that only part of it produces a response. It is then assumed that, after prolonged selection of one kind of stimulation, the filter is likely to change its setting and thus fail to select the wanted signal because it is taking in information from some quite other part of the surroundings.

Each of these theories agrees well with certain facts but encounters difficulties in explaining others. One of the anomalies which remains to be explained concerns the effects of loud and prolonged noise on visual detection. It was shown by Broadbent, (1954) and by Jerison (1959) that such detection becomes worse in noise of 100 db. above 0·0002 dynes/cm.^{2}. Of the foregoing theories, only the last explains this easily: but Jerison (1957) has also shown that very slight changes in the visual task prevent noise from having any effect, and this means that the explanation cannot be adequate. Jerison showed that a man watching a single clock-face, on which a pointer would give a brief signal 26 times in a 1 hr period, did just as well in noise as he did in quiet; but under either condition his performance deteriorated with time. On the other hand, if he watched three such clocks simultaneously, his performance did not deteriorate with time but was worse in noise than in quiet. Thus the explanation of the effects of noise can hardly be the same as that advanced to explain the deterioration at the end of a watch in quiet. If both effects were due to the same cause, the same test should surely be sensitive to both.

The experiments which follow are intended to probe further into the differences between Jerison's two tasks. As will be seen, they do so by questioning the sharpness of the distinction between signals which are detected in this kind of task and those which are not. It is more consistent with the evidence to hold that many signals fall in an intermediate region so that the man performing the task is doubtful whether or not they are in fact signals: and he must carry out a kind of statistical decision before he reports them. The number of signals which he is regarded as detecting will depend upon the criteria which he adopts in making this decision. It will be shown that the criteria are affected in rather different ways by noise and by prolonged work in quiet. Although the changes are different they may both result in a fall in the number of apparent detections made by the man; but this result is more likely to happen as a result of noise when other conditions are such as to reduce the probability of it happening through prolonged work in quiet.

II. SIGNAL DETECTION AS A STATISTICAL DECISION

Table 1 shows a number of the differences in experimental conditions between the tasks used by Jerison (1957) and Jerison (1959). (Some of the essential features in fact come from more ample accounts of this research, given by Jerison & Wallis (1957) and Jerison & Wing (1957)). It is notable that the task which showed effects of noise produced far more false-positives than the other task did. That is, the man frequently reported signals which were not really there. This might be due to the greater difficulty of keeping a watch on three places simultaneously, or to the high frequency with which signals came. Both factors would make it rational for a man to risk occasional reports which might not be justified, for the sake of improving his score. Most earlier studies in this field, however, have tended to ignore any effect upon the detection of such a gamble, for the following reason.

If we adopt the premise that a stimulus is at any instant either above threshold or below it, a careful observer will report signals with a probability p_c which corresponds to the probability of the signal being above threshold. He will never report a signal which is not there. A less careful observer may gamble that a signal is present on a proportion of the occasions when he does not in fact see one. His apparent detection rate will then rise to $p_c' = p_c' + p_w(1-p_c)$, at the cost of his responding falsely to a proportion p_w of the occasions when no signal was present. Since $(1-p_c) < 1$, the rise in detection rate must be less than the rise in the false-positive rate (Fig. 1 (*a*)). In experiments of the kind we are discussing, real signals last only a second or so and occur only once every couple of minutes. Yet it is usual to find only one or two false positives in a watch of an hour or more. Thus the value of p_w must be less than 0·001. Even in Jerison's three-clock experiment it is less than 0·01, and effects on p_c from changes in p_w would seem to be negligible. On the traditional theory, then, the number of false-positives found in these experiments is quite unimportant. The theory just given is of course the basis of the 'guessing correction' frequently employed in other connexions.

Table 1. *Differences between the tasks used by Jerison* (1957) *and Jerison* (1959)

Effects of noise	Trend during watch	No. of clocks	Signals/ hr.	Signals detected (%)	No. of reports which did not follow a signal
Fewer detections	None	3	165	Approx. 30	About 30/hr.
Same detections	Downwards	1	26	Approx. 60	Very infrequent: majority of subjects less than 4/hr.

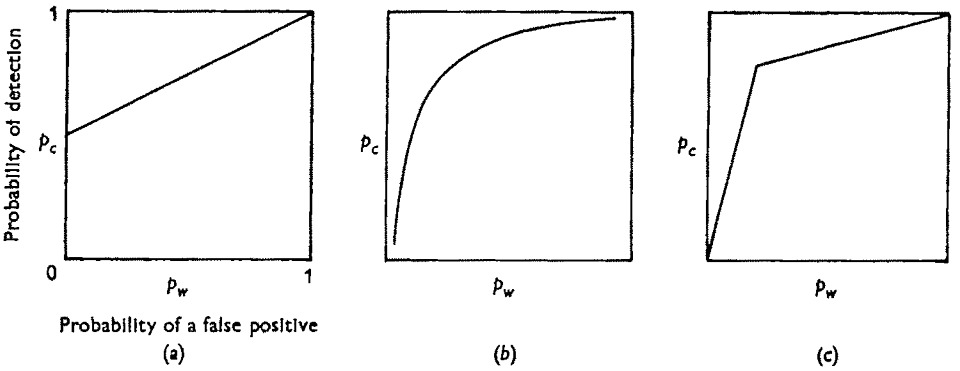

Fig. 1. The theoretical relationship between detection rate p_c and false-positive rate p_w on the traditional model of determinate thresholds (*a*), on a decision model (*b*) and on Luce's suggestion of determinate threshold with random refraining from response (c).

Other theoretical assumptions are possible, however. It was suggested by Tanner & Swets (1954) that the occurrence of a stimulus at the sense-organs produces a change in the average value of some quantity which is varying randomly within the brain: so that any particular value of that quantity is more likely to be exceeded when a stimulus is present than when it is not. If some value is chosen as a criterion, the brain might produce the effect which we ordinarily call 'perceiving' the signal on any occasion when this criterion is exceeded. Because of the random variation, not all the presentations of a signal will result in detection: and sometimes the man will report awareness of a signal when none is in fact present. Factors which shift the criterion will cause a change in the detection rate p_c, and also in the false-positive rate p_w. On this theory, however, the change in p_c for a given change in p_w may be very large indeed. Suppose, for instance, that the random process within the brain is normally distributed, and its variance remains the same when the average is changed (Fig. 2). The change in probability of a detection produced by a change in criterion is then proportional to the integral of the normal distribution between the limits set by the old and new values of the criterion; and this is much larger for a given shift near the mean of the distribution than it is near the extremes. Thus a small change in a small value of p_w, corresponding to a criterion near the tail of the no-signal distribution, could well produce a large change in p_c if the same criterion was nearer the mean of the signal distribution. In Fig. 2 the area intersected between the criteria A and B is large under the right-hand curve but small under the left-hand curve. As a result the relationship of p_c to p_w takes on the form shown in Fig. 1 (*b*) where p_c rises very rapidly when p_w is increased for a small value. Thus the theory that detection results from a statistical decision can be experimentally distinguished from the theory that signals fall into sharply distinguished classes of detected and undetected with no intervening region of uncertainty. The distinction between the theories is tested by a study of the relation between detections and false positives. Such studies have been carried out in the case where the time of arrival of the signal is fairly well known and the observers are a few highly trained people; and the results in general favour the statistical theory (Swets, Tanner & Birdsall, 1961). The present data will show that the same conclusion holds for large numbers of naive observers in a very free situation.

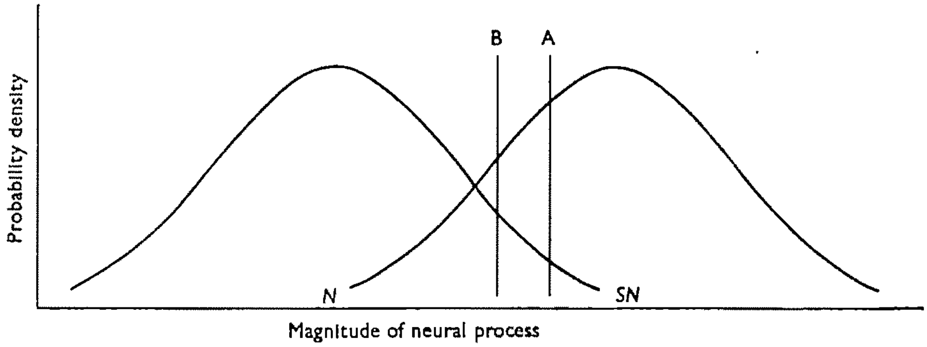

Fig. 2. A theoretical representation of the distribution of some process within the brain in the absence of a stimulus (curve *N*) and in the presence of a stimulus (curve *SN*). A and B represent criteria which may be set for the detection of the signal.

Assuming for the moment the applicability of the statistical theory, two quantities should be defined. The first of these is a measure of the signal–noise ratio, or extent to which the mean of the randomly varying process is shifted by the arrival of a signal. The measure usually taken is known as d', and is the distance between the signal and non-signal means, divided by the standard deviation of each distribution. It can be determined by observing empirical detection and false positive rates and then referring to a table of normal deviates. The second quantity is a measure of the degree of caution with which the criterion is placed, and is known as β. It is the ratio of the ordinate of the signal-present distribution to the ordinate of the signal-absent distribution, at the point where the criterion is placed. It can be determined either by observing empirical detection and false-positive rates and consulting tables of the normal distribution, or by shifting the criterion in some way and observing the ratio of the change in detections to the change in false-positives. It is important that β is theoretically independent of d'; for rational behaviour it is affected only by the relative importance which the observer places on detecting signals as opposed to avoiding false-positives, and by the probability which he assigns to the occurrence of signal as opposed to non-signal (see Swets *et al.* 1961). These two quantities d' and β therefore can be used to decide whether a condition such as the presence of loud noise is changing the efficiency of the observer's sensory system, or the degree of caution with which he is prepared to act. A drop in detections alone can be due to a decrease in d' with constant β, or a rise in β with constant d'.

III. EXPERIMENTS ON SIGNAL DETECTION IN PROLONGED WATCHES

Procedure

(*a*) *General.* In each of these experiments subjects were instructed to keep watch for a particular signal over periods of half an hour or more and to report if at any time they had the slightest suspicion that a signal might be present. Their report was given by pressing a button; having done this, they then had to indicate their degree of confidence that a signal was indeed present. In the auditory experiments they did this by putting a pencil tick in one of four columns on a piece of paper. The left-hand column was headed 'Sure' and was to be used if the subject was certain that a real signal was present. The next column was headed 'Not Quite Sure', the next 'Uncertain Either Way' and the fourth 'Not Quite Sure Not'. In the visual experiments four push buttons replaced the four columns.

All subjects were Naval Ratings aged between 18 and 30; none was used in more than one experiment. Each man was tested individually: each had normal hearing by audiometry.

(*b*) *Auditory experiments.* In these, each man's audiometric threshold at 1000 cyc./sec. was found on the right ear using a Marconi TF 895 audiometer. The threshold was taken as the lowest 5 db. step at which the listener could follow consistently as the tone was turned on and off using the silent switch of the audiometer. The attenuator was then left at this setting, and after instruction the subject was left for 45 min. during which occasional 1 sec. bursts of tone were inserted. One group of ten men received seventy-two signals during the period, and another group of eleven men received eighteen signals. In the former case the signals were spaced at intervals ranging from 4 to 153 sec., and in the latter case from 25 to 299 sec. In both cases the pattern of signals was identical in each 15 min period, but as there was no break from the subject's point of view he could not perceive this: it allows comparison of the beginning and end of the watch without contamination by the effects of inter-signal interval.

The instruction period before the main watch consisted of a verbal description of the test, a demonstration of the 1 sec. signal and a short period of 1½ min. in which six signals were inserted. After this practice period the subject was told how well he had done.

The whole experiment took place in an ordinary laboratory room which was reasonably quiet but not especially shielded against noises from outside. The experimenter stayed in the room with the subject, but sat behind him out of sight.

(c) *Visual experiments.* In these, the man sat at a table on which a black panel stood vertically at a distance of 30 in. from his eyes. On the panel were three fluorescent tubes, each arranged vertically and on a level with the others and each separated by 12 in. from its neighbour. These tubes flashed rhythmically and simultaneously once every 1·4 sec. for 0·30 sec. A signal was one slightly brighter flash, which might occur on any of the three tubes. The brightness of the background was 0·16 ft.-lamberts and of the flashing tubes 130 ft.-lamberts. A signal flash was 180 ft.-lamberts.

Again two groups were tested, but in this case both groups received the same signal frequency of thirty-six signals per ½ hr., the range of inter-signal intervals being from 20 to 80 sec. (This was in fact the schedule of signals used by Jerison in one of his three-clock experiments.) In one group of thirteen subjects, however, the watch consisted only of a ½ hr., while in the other group of twenty subjects it lasted 80 min. In either case the subject worked for two such periods, one on each of two successive days. On one day he was in an environment of 100 db. noise while on the other the same noise was present at only 75 db. (which will be called 'Quiet'). The order of conditions was counter-balanced between subjects. The noise consisted of amplified valve noise at approximately equal level per octave 100–5000 cyc./sec. It was delivered through a pair of loudspeakers mounted in the wall of the experimental room, which was windowless and insulated from outside sounds although internally reverberant. The experimenter was in this case outside the room, but could observe the subject through a periscope.

The practice given to the subject was in this case more thorough. On the day before the experiment he was given 10 min. practice in 75 db. noise, twelve signals being presented in this time. Two flashes after each signal a bell was rung to confirm any report the subject had made, or to draw his attention to his failure if he had missed the signal. At the end of the period his score of detections and false positives was given to him, and if he had only used one category of confidence he was asked to use more in the main experiment.

Before each day's experiment, the signal was demonstrated for each of the three lights to make sure that its brightness was remembered; and the subject then had to detect one signal correctly from each light before the main session started.

After the conclusion of the second day's experiment, eighteen of the subjects were asked the following question. 'If the loud noise represented 100 units of annoyance, how many units was the quieter noise?' It was explained that this meant giving a number near 100 if the two conditions didn't seem to differ much in annoyance, and a number very different from 100 if they were thought very different.

RESULTS

(a) *Applicability of decision theory.* The following analysis is intended to show that the reports which the subject gives with an intermediate degree of confidence are in fact more accurate than they would be on a basis of chance, but less accurate than those given with complete confidence. This shows that there is a genuine intermediate class of partly perceived signals whose inclusion or exclusion as detection will affect the apparent performance of the subject. In the first place we should consider the first 15 min. of the auditory experiments, the whole of the ½ hr. visual experiments in quiet, and the first ½ hr. of the 80 min. visual experiments in quiet, to see whether decision theory applies under conditions when the effects of prolonged work or noise should not appear. This is done by considering all responses assessed as 'Sure' and treating these as the result of performance with a cautious criterion; then considering all responses assessed as 'Sure' or 'Not Quite Sure', and treating these as performance with a slightly less cautious criterion, and so on. (This technique is due to Egan, Schulman & Greenberg, 1959). Whichever criterion we are considering, the detection rate is obviously the proportion of signals which are followed by response. It is harder, however, to give a numerical measure for the proportion of false-positives, because although we know the number of such reports that occurred, we do not know what to use as the total number of occasions of which this number is a proportion. That is, we do not know how long a period is regarded by the subjects as a single presentation which may be signal or non-signal. Arbitrarily, the convention has been adopted that in the auditory experiments we can roughly consider each period of one second as a presentation of no-signal, while in the visual experiments we can consider each pair of flashes on each light as one presentation of no-signal. (The latter assumption is based on the fact that many responses occur not after a signal but after the next no-signal flash: these are counted as responses to a signal). The assumptions are very crude, but as will be seen this does not affect the conclusions drawn.

Two results are completely clear: first, the ratio of detections to false-positives becomes steadily less as responses of lower confidence are included. Out of the whole fifty-four subjects there was only one man who showed the opposite change between the 'Sure' and 'Not Quite Sure' categories, seven men between the 'Not Quite Sure' and 'Uncertain' and nine men between the 'Uncertain' and 'Not Quite Sure Not'. Thus $P < 0{\cdot}02$ for every step along the rating scale. In other words, the rating of confidence is not assigned at random; the subjects can assess accurately the worth of their own judgements. Secondly, the increase in detection rate is normally greater than the increase in false-positive rate. Not one man showed the opposite relationship between 'Sure' and 'Not Quite Sure', nor between 'Not Quite Sure' and 'Uncertain.' One man did so between 'Uncertain' and 'Not Quite Sure Not'. Once again therefore the effect is significant, $P < 0{\cdot}001$. Thus the less confident judgements are not random, but are correct more often than chance alone would allow.

It should be noted that this relationship is so marked that the assumptions used in calculating false-positive rate do not matter. If, for instance, in the visual experiment it were to be assumed that every four flashes were to be regarded as a presentation of no-signal, rather than every two, so that the calculated false-positive rate was doubled, there would still be no men showing an increase in false positive rate greater than that in detection rate between 'Sure' and 'Not Quite Sure', only one man between 'Not Quite Sure' and 'Uncertain', and only two men between 'Uncertain' and 'Not Quite Sure Not'. The effect is too large for such minor adjustments to alter it.

These two findings, while very simple, are completely inconsistent with the usual correction for false positives based on random guessing: or for that matter the more sophisticated theory of random failure to respond, suggested by Luce (1960), and discussed below.

On the other hand if we calculate *d′* for each subject from the most cautious criterion and from the most risky one, we ought on the decision model to find that both are identical. In fact we obtain a pooled τ correlation of 0·55 between the two measurements. This indicates reasonable agreement with the model based on decision theory, particularly when it is remembered that the presence or absence of one false-positive in the 'Sure' category makes a very large difference to the calculated *d′*: but nevertheless this correlation really means only that the hits and false-positives at the cautious criterion are correlated with those at the risky one, which is to be expected on most assumptions. A more striking test is to calculate β twice for each subject, once from the ordinates of the normal distribution corresponding to his performance in the 'Sure' category, and once for the ratio of hits to false-positives in all the other three categories combined. (The latter method of calculation only gives a quantity proportional to β, since the subjective probability of a signal is unknown.) In this case the two methods of calculation are based on data which are quite different, but the pooled τ correlation is 0·52, for which $P < 0{\cdot}001$.

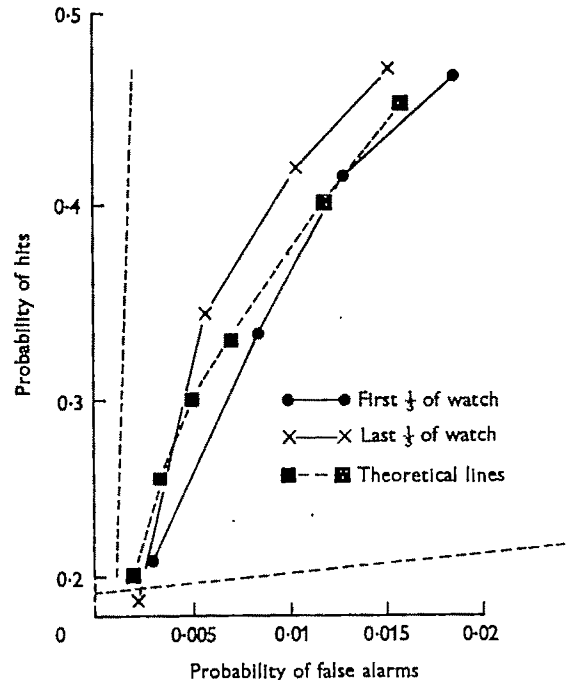

Fig. 3. Performance summed over all subjects for the visual vigilance task performed for 80 min. under quiet conditions. Data are shown separately for the first ¼ hr. (approximately ¼) and for a similar period at the end of the watch. The dotted line adjacent to the experimental results represents performance for a constant value of *d′*. The other two dotted lines represent the slopes of the two portions of the model of Luce (1963) as illustrated in Fig. 1 (*c*). It can be seen that no combination of only two lines with these slopes will fit the experimental results obtained.

Thus it seems clear that the results will not fit a theory that stimuli are either definitely detected or else definitely missed; while the findings are reasonably consistent with the view that detection is a process of statistical decision.

(*b*) *Trends during the watch.* The following analysis is intended to show that as the watch proceeds the man becomes relatively less likely to say that he is 'Sure' of the presence of a signal: but does not show a corresponding increase in the number of signals not reported at all. In technical terms, the point to be established is that when β is high (cautious performance with few false-positives) it becomes even higher towards the end of the watch; whereas when β is low (risky performance with many false-positives) there is no change during the run.

When the first $\frac{1}{4}$ hr. of the auditory task is compared with the last, or when the first $\frac{1}{2}$ hr. of the long visual watch in quiet is compared with the last, no statistically significant difference in detection rate appears. This applies whether a cautious or a risky criterion is considered. It will be noted, however, that some deterioration does consistently appear in performance at the most cautious criterion, even though this may be due to chance: and that no such consistent deterioration appears at the least cautious criterion.

If d' is calculated for the beginning and end of watch, again there is no statistically significant change. Calculating d' from the risky criterion, because of the scant evidence from false-positives at the cautious criterion, seven subjects in the auditory experiments show an upward trend in d' and fourteen subjects a downward one. For the visual task the corresponding numbers are 11 and 8.

When β is calculated for the most cautious criterion, however, a marked change during the run is observed. In the auditory experiments sixteen men show a rise in β during the watch, and only two a fall ($P < 0{\cdot}002$). In the visual experiment twelve men show a rise and four a fall ($P < 0{\cdot}05$ one-tail). (It should perhaps be noted at this point that a difficulty in assessing β occurs when no false-positives are made in either the beginning or end of the run. In that case the assumption was adopted that a smaller number of detections represented a rise in β, i.e. increased caution. Since no change in d' was present on the average this assumption seems reasonable.)

Thus increased caution appears as the watch proceeds, without any change on average in the signal–noise ratio within the nervous system. The reason why this increased caution is not significantly reflected in decreased detection is that several men showed an improvement in d' during the run, which they expressed by reducing their false-positive rate without reducing their detection rate. There was even one case of a man who increased both rates and yet showed increased β; this is possible if d' improves when the detection rate is less than 50%, and the point is important when considering the relationship of these results to earlier ones in the literature. Clearly detection rate will not always decrease during the run, nor will false-positive rate.

When the most risky criterion is considered, however, no significant change in β appears: in the auditory experiments eleven men got more cautious and ten less, while in the visual experiment the numbers were eleven and six. Thus taking all the subjects together we can say that the cautious performance became more cautious during the run, while the risky performance did not.

Table 2. *Median number of hits and of false-positives in each category and each condition (with the semi-inter-quartile range in brackets.)*

(Column 1 indicates responses in the category 'Sure', Column 2 'Not Quite Sure', and so on. Since medians do not add up to the total number of signals, the median number of omissions is also given.)

	No. of signals	Hits					FPs				Hits					FPs			
Auditory vigilance	in period	1	2	3	4	Om.	1	2	3	4	1	2	3	4	Om.	1	2	3	4
		1st 15 min.									Last 15 min.								
High frequency	24	13·5	3	1	0	5·5	0	1	0	0	13·0	3·5	0·5	0	4	0	0	1	1
		(5·0)	(2·0)	(1·0)	(0·5)	(3·5)	(0·5)	(1·0)	(6·5)	(3·0)	(6·0)	(1·0)	(1·5)	(0)	(3·5)	(0)	(0·5)	(2·0)	(1·0)
Low frequency	6	5	0	0	0	1	1	3	3	0	3	1	0	0	2	0	3	2	2
		(1·5)	(0·5)	(0)	(0)	(0·5)	(9·0)	(5·5)	(2·0)	(1·5)	(2·5)	(0·5)	(0·5)	0	(1·5)	(5·0)	(3·0)	(2·5)	(2·5)
Visual vigilance Quiet 30 min	36	7	5	2	2	19	3	4	6	4	—	—	—	—	—	—	—	—	—
		(3·75)	(2·0)	(2·25)	(1·5)	(4·75)	(5·5)	(9·75)	(10·75)	(8·25)	—	—	—	—	—	—	—	—	—
		1st 30 min.									Last 30 min.								
Quiet 80 min	36	7	4·5	2·5	1·5	18·5	0	4	5·5	2·5	5	5	2	1	18	0	2	5·5	5
		(4·5)	(2·0)	(2·0)	(1·0)	(5·0)	(1·25)	(5·75)	(3·5)	(5·0)	(3·75)	(1·75)	(0·75)	(1·25)	(4·0)	(0·5)	(3·75)	(6·5)	(5·0)
Noise 80 min	36	6	5·5	2	1	18·5	0	4	7·0	4·0	5·5	3	3	1	20	0	2	2	2·5
		(3·0)	(3·25)	(1·5)	(1·25)	(3·5)	(0·75)	(4·5)	(5·5)	(5·75)	(4·5)	(1·75)	(1·25)	(1·5)	(4·0)	(0·25)	(2·5)	(5·0)	(4·0)

Just as in Jerison's experiments, performance under conditions producing few false-positives gave a change during the watch, while performance under conditions favouring many false-positives did not give such a change. As the differences were achieved without altering the signal rate or the number of sources of information, it appears that those variables are not necessarily the crucial ones.

(*c*) *Effects of noise on the visual task.* The following analysis is intended to show that, in noise, responses tend either to be classed as 'Sure' *or* not to occur at all, with relatively fewer in the intermediate categories of confidence. This means that it is unlikely that the number of correct 'Sure' responses will drop in noise; if there is a deterioration of any kind, it would be shown by an increase in the number of failures to respond at all when a signal was nevertheless present. Thus the effect is almost opposite to that of prolonged work in quiet, although not completely so. In technical terms, the point to be established is that the value of β for the cautious criterion (high β) becomes more similar to that for the risky criterion (low β) when loud noise is present.

The comparisons of greatest interest are between the last ½ hr. in quiet and the last ½ hr. in noise of the 80 min. watch. Jerison's effect of noise applies only at the end of the task, and the same feature has been found in a number of other experiments (Broadbent, 1954, 1962). In the present research no significant differences were noted between the first ½ hr. of the 80 min. in noise and in quiet, nor in the short visual experiment.

Considering then the last ½ hr., we find that once again the raw detection rate shows no significant effect of noise although there is a statistically insignificant deterioration at the most risky criterion (Table 2). No such deterioration appears at the most cautious criterion.

If d' is calculated from the most cautious criterion there is no change in noise; but it must be admitted that a calculation of d' from the most risky criterion does show a significant improvement in noise ($P < 0{\cdot}05$). The most striking feature of the results, however, is that the risky and cautious criteria seem to be closer together in noise. A measure of the separation between criteria can be obtained by taking the ratio of hits to false-positives in the 'Not Quite Sure' category as an index of β for the cautious criterion, taking the same ratio for the 'Not Quite Sure Not' category as an index of β for the risky criterion, and dividing the first by the second. The larger this quantity, the more widely separated the criteria. (Division rather than subtraction is thought appropriate, because since there are usually more false-positives than hits in the 'Not Quite Sure Not' category, the index for the more risky criterion normally lies between 0 and 1, and a difference measure would simply represent the value of the index for the more cautious criterion. However, it makes little difference to the results given below whichever is used. It should also be noted that if a category was not used at all by a subject the 'Uncertain' category was substituted in the calculation: while if there were hits but not false-positives or vice versa, a value of ½ was entered in the empty cell to avoid infinite values. Instead of ½, any other value less than one could have been used without affecting the results given below.)

Of the twenty subjects, sixteen showed a smaller difference between criteria in noise: thus $P = 0{\cdot}012$. The responses tended therefore in noise to be confident assertions or else denials of the presence of a signal, with relatively fewer intermediate reports.

This effect does not necessarily result in lowered detection, since some individuals bring the criteria closer together by increasing the number of signals they correctly report as 'Sure' rather than increasing the number they incorrectly fail to report at all. Taking the group as a whole, neither the most cautious nor the most risky criterion shows a statistically significant change in β.

It seems fair to conclude that the effect of noise is rather different in kind from that of prolonged work: the relationship to Jerison's results can best be considered after an examination of the individual differences in effects of noise.

(*d*) *Individual differences in effects of noise.* As already indicated, one must distinguish between the tendency in noise to avoid the middle categories and the tendency to detect fewer signals in noise. The person who shows one of these effects most markedly is not necessarily the one who shows the other effect to the greatest extent. The first effect, of avoiding the intermediate categories of response, is greatest in those men who reported the most annoyance during the noise experiment. If we calculate the distance between the criteria along the horizontal axis of Fig. 2, by considering the difference in false-positive rates between the criteria, we can measure the reduction of this distance in noise as compared with quiet by the difference of the differences. There is a τ of $-0{\cdot}40$ between this quantity and the numerical estimate which the particular subject gives for the annoyance due to the 'quiet' condition, taking the annoyance due to noise as 100. This correlation is significant $P < 0{\cdot}05$. Thus the man who says he was much more annoyed by noise than by quiet tends also in noise to avoid the intermediate categories of response. It must of course be remembered that the estimate of annoyance may possibly be due to the man's knowledge of his own performance on this occasion, rather than the performance being caused by the annoyance.

It is perhaps more interesting to identify those individuals who bring their criteria together in noise by increasing the frequency of complete failures of response rather than by increasing their 'Sure' ones: that is, the people who detect fewer signals in noise. There is no correlation between annoyance and the size of the drop in detections in noise. The latter measure is, however, correlated with the total number of false-positives taking both runs together: $\tau = 0{\cdot}39$, $P < 0{\cdot}05$. That is, the people who show most false-positives are the ones who show the biggest drop in detections in noise.

This individual difference is consistent with the effect of noise on the whole group, discussed in the last section. If responses of intermediate confidence are avoided in noise, any drop in detections in noise will be greater when measured at the risky criterion. Correspondingly, those individual subjects who perform most riskily are the ones who detect fewest signals in noise. It would appear therefore that conditions which favour false-positives will also favour the occurrence of a drop in detections when loud noise is present. This again shows a similarity to Jerison's results: in his experiments it was the task in which false-positives were frequent which showed a drop in detections in noise. Admittedly he found such a drop to occur over the group as a whole, whereas we have shown it only in some individuals: but he used a very different type of subject, and this may be the cause of this discrepancy.

IV. DISCUSSION

The applicability of decision theory. As already indicated, in so far as the present results show that increases in false-positives do not result from random guessing, they confirm the view already advanced by American workers. The method of testing the theory is, however, complementary to earlier studies, because relatively large numbers of naive subjects have been used instead of a few highly trained observers. The present method tends to give crude results, but it is free from suspicion that those results are peculiar to particular observers or methods of training. Whichever type of experiment is thought most convincing, there can now be little doubt that p_c does *not* increase by an amount equal to $p_w (1 - p_c)$ as conditions change in such a way as to increase p_w.

In addition it can be claimed that these results invalidate another interpretation which is at variance with that based on decision theory, and which was still tenable in the light of earlier findings. This was the suggestion made by Luce (1960, 1963) and illustrated in Fig. 1C, that there is a 'true' level of performance giving detection rate p_c and false-positive rate p_w; from which random guessing may improve the hit rate to $p_c' = p_c + (p_w' - p_w)(1 - p_c)$ for a false-positive rate of p_w'. Random refraining from response may reduce the hit rate to $p_c'' = (1/h)p_c$, for a false-positive rate $p_w'' = (1/h)p_w$. As is noted by Swets (1961), the resulting pair of intersecting straight line functions fit most existing data about as well as the predictions from the Tanner and Swets model do. On the suggestion pointed out by Luce, however, when one compares performance at two levels of false-positive rate, *either* the ratio of detections to false-positives will remain constant *or* the rise in detection rate will be less than the rise in false-positive rate *or* the point of 'true' performance lies between the two points observed. Since our results exclude the first two alternatives when 'Sure' responses are compared with 'Sure' + 'Not Quite Sure' responses, this implies that those two levels of false-positive rate bracket the 'true' level. But our results also show a rise in hit rate greater than the rise in false-positive rate when the 'Uncertain Either Way' responses are included: and even when the further step of adding 'Not Quite Sure Not' is taken. Therefore Luce's suggestion is not applicable in its simple form. More complex versions of his model can be devised to include the present results, but it seems to the present authors that they would be tantamount to adopting the approach of Tanner and Swets.

It can therefore fairly be held that the traditional assumption, that the human subject either definitely reports or definitely fails to report stimuli, must at this stage be finally abandoned. Amongst other consequences, this conclusion implies that one must not in future discuss or report threshold values in psycho-physical work, since tiny changes in false-positive rate will have an enormous effect on the stimulus intensity needed for any particular level of detection. The conclusion is supported by the way in which parameters calculated from decision theory have in these experiments shown effects of prolonged work and of noise when orthodox measures of detection rate have not. The failure of detection rate as a measure is probably due to the improvement of d' with practice amongst our relatively untrained subjects. While this improvement has not so far been mentioned, as outside the main stream of the results, d' for the second session was normally much larger than for the first session.

Interpretation of the changes during a watch or in noise. At the empirical level, these results show that Jerison's curiously different findings with his two tasks can be reconciled under the single generalization that cautious performance is more likely to show effects of prolonged work and risky performance more likely to show effects of noise. But the interpretation remains obscure. Of the theories mentioned initially, expectancy has a niche within decision theory, since if behaviour is rational β will reflect the relative probabilities of signal and non-signal. The undoubted effects of signal probability (Colquhoun, 1961) are therefore intelligible; but both risky and cautious criteria ought to move in the same direction when expectancy changes, and in these results they do not. Distraction does not *a priori* seem more likely to affect one criterion than another; and a separate experiment has in fact suggested that a deliberately produced division of attention affects d' rather than β (Broadbent & Gregory, 1963).

There remains arousal, but at first sight one might have expected changes in the general level of efficiency of the nervous system to alter d'. It has, however, been pointed out by Welford (1962) that a change in responsiveness of the whole brain might well increase both the neural activity resulting from a stimulus and the random activity occurring in the absence of a stimulus, in such a way as to leave d' unchanged. If, however, a criterion for response remained fixed at a certain level of neural activity, β would increase as arousal declined: and if two criteria were separated by a fixed amount of neural activity, they would, when arousal increased, move closer together in terms of β. Thus changes in arousal might well give either of the changes in β which we have observed. A link with arousal is also particularly attractive because quite separate studies (Broadbent, 1963) lead one to conclude that noise has its bad effects through over-arousal.

Unfortunately, however, the effects of noise and of prolonged work are not in these results exact opposites as has already been noted. We do not observe an increased separation of criteria at the end of the watch period, nor a decrease in β for the cautious criterion in noise. Thus it is difficult to account for both sets of findings in terms of a single concept of arousal. It might be done, however, by distinguishing various kinds of arousal.

An example of one hypothesis which would do this may be of interest. In an analysis based on decision theory, there is a mathematically ideal position for the criterion, which can be calculated if one can measure the gain which the subject obtains from detecting a signal (which we may call G_H) and the loss which he experiences from making a false-positive (L_{FP}). At the optimal position of the criterion, β will be proportional to L_{FP}/G_H. It will be obvious that when a man cares more about avoiding false-positives than about making hits, β will be high, which we have already considered as the mark of a cautious criterion.

Now it is likely that various environmental conditions may affect positive incentives differently from negative incentives: drugs, for example, sometimes reduce response to the latter without affecting the former. If, for instance, noise increased both G_H and L_{FP} by equal amounts the ratio of one to the other would become more nearly unity whether or not it was originally greater than one. In such a case β for cautious criteria would more closely resemble β for risky criteria.

If, on the other hand, prolonged work reduced G_H by a greater amount than it reduced L_{FP}, the change in β would be greater when G_H was relatively small (cautious criteria) than when it was relatively large (risky criteria). On such an interpretation therefore it is reasonable that the effects of the two stresses should be partly opposed (since one raises and one lowers motivation) and partly quite different (since one affects all kinds of motivation and the other does not).

Other analyses are of course possible: but it should be possible to distinguish between them by further experiments. The foregoing speculation makes it clear, however, that a division of arousal into two varieties could be expected to explain the present results.

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CEREBRAL DOMINANCE IN REACTION TIME RESPONSES

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Previous experimental work has suggested that bilateral reaction times to visual stimuli may be subject to dominance effects, and that where pairs of stimuli are closely ordered in time, these effects may be more complex than a simple superiority in speed of performance by the dominant hand. Twenty subjects, ten markedly left handed and ten markedly right handed, were selected from a larger group of forty-four subjects to examine these effects in detail.

Results showed:

(1) For single responses and paired responses to simultaneous stimuli, responses by the dominant hand were made significantly faster than by the non-dominant hand.

(2) A reversal of this effect occurred with the second responses to paired stimuli which were separated by an interval of 100 msec., the dominant responses being made significantly more slowly than the non-dominant responses.

(3) This reversal was a feature of the delayed responses only, i.e. of those responses where reaction time to the second stimulus was lengthened following a response to the first stimulus. Where the response to S 2 was not delayed, dominance effects were in the same direction as for the single responses.

(4) A subsidiary finding was that, apart from the relative magnitudes of response times, *types* of response to paired stimuli showed some relationship to individual dominance. In particular the response to S 2 was more often delayed in the stimulus order non-dominant/dominant than in the opposite order and responses were more often made independently in the order dominant/non-dominant.

I. INTRODUCTION

In a series of reaction-time experiments designed primarily to test a theory of the psychological refractory period put forward by Welford (1952), it was observed that different subjects dealt with the experimental task in different ways. It was also observed that an individual might show more than one pattern of response in relation to one experimental procedure, and, further, that there was evidence with some subjects that the response made was determined in part by laterality preferences. Where stimuli were presented in a bilaterally symmetrical design the pattern of responses adopted by the subjects did not show the same symmetry and the type of response elicited depended in part on the order in which the left and right hands were called upon to respond. In one right-handed subject this effect was marked; delays in responding to a second stimulus being frequent only when the stimuli were presented in the order left/right (Elithorn & Lawrence, 1955; Halliday, Kerr & Elithorn, 1960). Varnum suggested in 1934 that the study of simple reflex or near reflex responses might throw some light on the problem of the cause of dominance phenomena in more complex skills; Varnum's own study with patellar reflexes failed to reveal any differences attributable to dominance phenomena, while previous work with reaction-time key pressing responses has given conflicting results. In the present experiment we have studied single and double reaction times and analysed the responses in terms of the subjects' existing laterality preferences.

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II. APPARATUS AND EXPERIMENTAL DESIGN

The apparatus and the methods of programming stimulus situations and recording response times have previously been reported (Halliday *et al.* 1960). The task required key-pressing responses to visual stimuli, presented singly or in pairs, on the left and right of a central fixation point. The subject responded by pressing with his left or right hand the key on the side corresponding to the side of the stimulus. A warning light placed centrally between the two stimulus lights was illuminated 1200 msec. before each test situation and remained on for 800 msec. Stimulus situations were presented regularly every 4 sec. The experiment undertaken by each subject consisted of 8 runs: 4 control runs, alternating with 4 experimental runs. Each control run consisted of the presentation of 12 single stimuli on the left or right, either immediately the warning period ended or 100 msec. later ('Time 0' or 'Time 100'). In the experimental runs 12 pairs of stimuli were interspersed at random with 12 single stimuli. The paired stimuli occurred on the left and right, and were presented either simultaneously or separated by an interval of 100 msec. Half the simultaneous pairs were presented at 'Time 0' and half at 'Time 100'.

Three factors have, therefore, been systematically varied in this experiment: side of presentation (left or right), time of presentation ('Time 0' or 'Time 100'), and the complexity of the task (one response or two). A fourth important source of variance is the difference between individual subjects.

The number of responses made by each subject was 192 (48 in the control runs and 144 in the experimental ones). The first run of each type has been treated as practice, and the remaining 144 responses (36 + 108) have been analysed. The order of stimulus presentation in each run was randomized. In case order effects should affect the response times in an experiment of such limited duration, two different randomized series were used. In the subsequent analysis no significant order effects were observed, and the two sets of data have therefore been combined.

III. SUBJECTS

Ten subjects who were predominantly left handed and ten predominantly right handed were chosen from a larger group of forty-four volunteers, mainly medical or dental students and laboratory workers, on the basis that their laterality preferences were the most consistent. The tests of laterality used are given in Appendix I. On the basis of these tests all forty-four subjects were placed in rank order for right-hand preference and the results of those ranking 1–10 and 35–44 were selected for analysis in this paper. The sinistrality indices ranged between 0 and 0·1 and between 0·8 and 0·95 for the two groups. The subjects were adults of both sexes ranging in age from 19 to 41. They were inexperienced in reaction time tests but as a group were above average in intelligence and educational attainment.

Subjects were asked to respond as quickly as possible by pressing the key corresponding to the light which was illuminated. They were told which runs would contain single stimuli only, and which would contain both single and double stimuli; also that some of the paired stimuli would be simultaneous and some separated by a short interval. Apart from the instruction to respond to each stimulus as soon as it appeared and to try and avoid waiting, no attempt was made to influence subjects to produce any particular type of performance.

IV. RESULTS

The aim of the present paper is to examine possible sources of asymmetry in the responses made in a bilaterally symmetrical stimulus-response situation. As half the subjects were strongly left-dominant and half strongly right-dominant, analysis of the whole group in terms of both a left/right orientation and a dominant/non-dominant orientation should reveal the relative importance of these two sources of asymmetry. The results have therefore been analysed both in terms of the actual side on which the stimulus occurred, and in terms of the subjects' dominance.

Throughout the paper the terms 'dominant' and 'non-dominant' have been used to describe the preferred and non-preferred side and not the relevant cerebral hemisphere. A stimulus on the 'dominant' side of a right-handed person occurs therefore on the right.

Table 1 gives mean reaction times for the whole group. Table 2 summarizes the results of an analysis of variance, which was used to investigate the interactions between laterality preferences and other variables. Separate analyses have been made of the control runs, of the experimental runs at 'Time 0', and of the experimental runs at 'Time 100'. The split in the analysis of the experimental runs was dictated by the fact that the variance attributable to order (first or second) is confounded with that due to the time of presentation, since any stimulus which is the *first* of two always occurs at 'Time 0' and any stimulus which is *second* at 'Time 100'. This limitation, however, is not a serious one since a rigorous test of the order effect *per se* is not the aim of the experiment, our main interest in this variable being in its interaction with laterality effects.* The means of the responses to the simultaneous stimuli are included in Table 1, but have been excluded from the analyses of variance.

Table 1. *Means of individual responses in terms of both dominance and laterality in msec.*

	Both sides		Dominant		Non-dominant		Left		Right	
Control runs	Mean	S.E.	Mean	S.E.	Mean	S.E.	Mean	S.E.	Mean	S.E.
'Time 0'	288	3·4	280	4·5	296	5·0	294	4·6	282	4·8
				$P < 0{\cdot}01$				$P < 0{\cdot}05$		
'Time 100'	281	3·2	273	4·0	288	4·9	278	4·9	284	4·3
				$P < 0{\cdot}05$						
Mean for both times	284	2·3	277	3·0	292	3·5	286	3·4	283	3·2
				$P < 0{\cdot}001$						
Experimental runs										
(*a*) 'Time 0'										
Alone	341	4·6	338	7·0	343	5·9	340	6·3	342	6·4
First	361	7·0	359	9·5	362	10·2	356	9·6	365	10·1
Simultaneous	353	6·1	350	8·8	356	8·6	360	8·7	346	8·8
Mean for all three conditions	352	3·5	349	4·9	354	4·9	351	4·8	352	5·0
(*b*) 'Time 100'										
Alone	328	3·8	324	5·5	333	5·2	334	5·5	323	5·2
Second	403	6·9	416	10·1	391	9·2	405	10·2	402	9·3
				$P < 0{\cdot}05$						
Simultaneous	348	5·9	341	8·4	354	8·3	351	8·4	344	8·3
Mean for all three conditions	360	3·4	360	5·1	359	4·6	364	5·0	356	4·8

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* The comparative unimportance of the 'Time' factor as a source of variance compared with the order of the stimuli (first or second) is in any case easily seen from the means given in Table 1. The means for the simultaneously occurring stimuli show little difference between 'Time 0' and 'Time 100' (353 and 347 msec. respectively) while the means for the first and second stimulus-response situation for 'Time 0' and 'Time 100' are 361 and 403 msec. respectively.

Table 2.

	Analysis in terms of dominance			Analysis in terms of left/right orientation	
	Mean square	*F*	D.F.	Mean square	*F*
	(*a*) Control situation				
Side	153,760	21·76***	1	6,502	0·96
Situation ('Time 0' or 'Time 100')	31,080	4·40*	1	31,080	(4·36*)
Individuals	197,237	27·92***	19	197,237	27·64***
Side/situation	250	0·04	1	48,303	6·77**
Side/individuals	4,419	0·65	19	12,169	1·71*
Situation/individuals	6,918	1·02	19	6,918	1·02
Side/situation/individuals	11,310	1·66	19	8,780	1·29
Residual variance	6,805		80	6,805	
Revised†	7,065		138, 119	7,136	
	(*b*) Experimental situation				
'Time 0'					
Side	8,850	0·66	1	20,702	1·55
Situation (alone or first)	251,222	18·18**	1	251,222	18·06**
Individuals	700,864	50·72***	19	700,864	50·39***
Side/situation	903	0·07	1	10,081	0·75
Side/individuals	16,522	1·23	19	15,898	1·19
Situation/individuals	45,666	(3·30**)	19	45,666	(3·28**)
Side/situation/individuals	25,310	1·83*	19	24,827	1·78*
Residual variance	13,399	—	80	13,399	—
Revised†	13,818	—	101	13,909	—
'Time 100'					
Side	38,595	1·87	1	37,057	1·79
Situation (alone or second)	3,601,500	150·68***	1	3,601,500	142·26***
Individuals	538,694	22·54***	19	538,694	21·28***
Side/situation	193,558	8·10**	1	10,973	0·53
Side/individuals	31,966	1·55	19	32,047	1·55
Situation/individuals	113,021	4·73**	19	113,021	4·46**
Side/situation/individuals	28,646	1·39	19	38,256	1·67
Residual variance	20,677	—	80	20,677	—
Revised†	23,902	—	119, 120	25,316	—

*, ** and *** = significant at the 0·05, 0·01 and 0·001 level respectively. If in parentheses they are not significant in relation to lower order interactions.

† Final estimate of residual variance including non-significant factors.

(i) *Results in the control situation*

Side of stimulus

In the control runs the results are clear-cut. The analysis using dominance as a criterion shows that dominance effects account for a very appreciable proportion of the variance and are significant at the 0·001 level of confidence (Table 2*a*), the dominant hand responding faster on the average than the non-dominant hand.*

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* This effect is not due to one or two subjects. If, instead of examining the mean values for the whole group, the means for individual subjects are examined, the tendency to respond quicker with the dominant hand in the control runs is observed in eight out of the ten right-handed subjects and eight out of the ten left-handed subjects.

In contrast, when the laterality effect is tested in the left/right analysis it is found to account for a much smaller proportion of the variance and is not significant. There is, however, a significant interaction between 'Side' and 'Time' in the left/right analysis ($P < 0{\cdot}01$). This is seen in Table 1 as a mean difference of 12 msec. between the left- and right-hand responses at 'Time 0' (right response faster) which just reaches the 0·05 level of significance whereas at 'Time 100' the left response is faster but not significantly so. A significant interaction between 'Individuals' and 'Side' ($P < 0{\cdot}05$) confirms the dominance effect observed as a main factor, left-dominant subjects being faster in responding on the left and right-dominant subjects faster on the right.

The contribution made to the total variance by the two remaining factors—'Individuals' and 'Situation'—is of course substantially unaffected by the form of the analysis whether it is made in terms of left/right or dominant/non-dominant.

Individuals

It is apparent that individual differences are a major source of variance, reaching the 0·001 level of significance (Table 2*a*). This is a reflection of inter-subject differences in the over-all speed of response.

Situation

The third factor, 'Situation', which in the control runs represents the time at which the single stimuli occurred, is significant at the 0·05 level (Table 2*a*). Responses to the stimuli at 'Time 100', which have in effect a longer warning period, tended to be slightly faster (Table 1).

(ii) *Results in the experimental situation*

Side of stimulus

In the experimental runs the dominance effect is not so clear. Table 2*b* shows that it does not reach an acceptable level of significance as a main factor. Examination of Table 1, however, shows that at 'Time 0' all the differences between the dominant and non-dominant sides are in the expected direction. At 'Time 100' the tendency for responses to be made faster on the dominant side is still apparent when the responses are to single stimuli or to two stimuli presented simultaneously. However, when the stimulus on the dominant side is the *second* of the two, the mean reaction time is much *slower* (25 msec.) than the mean reaction time to the comparable stimulus on the non-dominant side. This difference is significant at the 0·05 level. This reversal of the dominance effect appears as a significant first-order interaction ('Side'/'Situation', $P < 0{\cdot}01$) in the analysis of variance (Table 2*b*). It reflects a reversal, when the stimulus is second, of the tendency consistently found in all the other comparisons for the dominant responses to be faster than the non-dominant responses.

In the left/right analysis the laterality factor is not significant either as a main factor or as a first-order interaction.

Individuals

The 'Individuals' factor is highly significant in the experimental runs ($P < 0{\cdot}001$) just as it was in the control runs. Apart from over-all differences in speed, however, this also reflects individual differences in the strategies adopted to deal with the more complex pattern of double stimuli. These differences in individual strategies are of considerable interest in relation to theories of psychological refractoriness and will be described in detail elsewhere. In the present paper they are discussed briefly in relation to dominance phenomena.

Situation

In the experimental runs the third factor, 'Situation', represents the presence or absence of another stimulus. This is clearly an important source of variance (Table 2*b*). At 'Time 0' where the stimulus occurs either *alone* or as the *first* of a pair, the effect of another stimulus (i.e. the second) is to lengthen reaction time to the first by an average of 20 msec. (Table 1). As a source of variance, this yields an F ratio of 18·2 ($P < 0{\cdot}01$). This effect is independent of dominance phenomena as is seen by the insignificance of the F value for the 'Side/Situation' interaction.

At 'Time 100', however, the mean reaction time to a stimulus which is the *second* of two is 75 msec. longer than the mean reaction time to a single stimulus. This factor (i.e. the presence or absence of a first stimulus) is the most important source of variance. ($F = 150{\cdot}7$, $P < 0{\cdot}001$). It is not, however, independent of dominance, since, as has already been mentioned, the Side/Situation interaction is significant when analysed in terms of dominance but not when analysed in terms of left and right. The unexpectedly high value for the second response when made on the dominant side is the main cause of this significant interaction.

(iii) *Strategies in relation to dominance*

The significant interaction observed between 'Situation and Individuals' arises from the variation in the way in which subjects respond to the second stimulus. This reflects differences in strategies used by the subjects in dealing with the paired responses. The task required of our subjects is deceptive in its apparent simplicity. It is generally held that it is impossible to attend simultaneously to two tasks and that apparent *simultaneity of performance* is due either to a rapid alternation of attention or to the fusion of the two performances. In the present situation different patterns of responses may be interpreted as the outcome of different strategies (Elithorn, Kerr & Mingay, in preparation). Only two of the seven types of response described in this paper can be shown to be related to dominance phenomena. These two types of response are those classified as 'independent' and 'delayed'. 'Independent' responses are those in which the responses to both S 1 and S 2 fall within the range of the corresponding responses in the control runs—excluding the highest and the lowest and omitting premature responses.* 'Delayed' responses are those in which S 1 is within the control range as defined above but in which S 2 is longer. Both types of response and the extent to which they can be affected by a subject's handedness are illustrated in the scattergrams discussed below.

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* This criterion is one which errs very much on the side of strictness; not only does it exclude reaction times at the extremes of the control range but since single responses in the experimental runs are on the average considerably longer than in the control series, it is likely therefore that second responses could also be longer and still be independent.

Fig. 1 shows the scatter of responses to stimuli given in the left/right order (crosses) and right/left order (circles) for two subjects, one right handed and one left handed. Fig. 1 *a* shows that for the right-handed subject responses to the second of two stimuli presented in the order left/right may be markedly delayed, whereas two stimuli presented in the reverse order are performed equally fast with no apparent interference to the second response from the first. The left-handed subject (Fig. 1 *b*) made

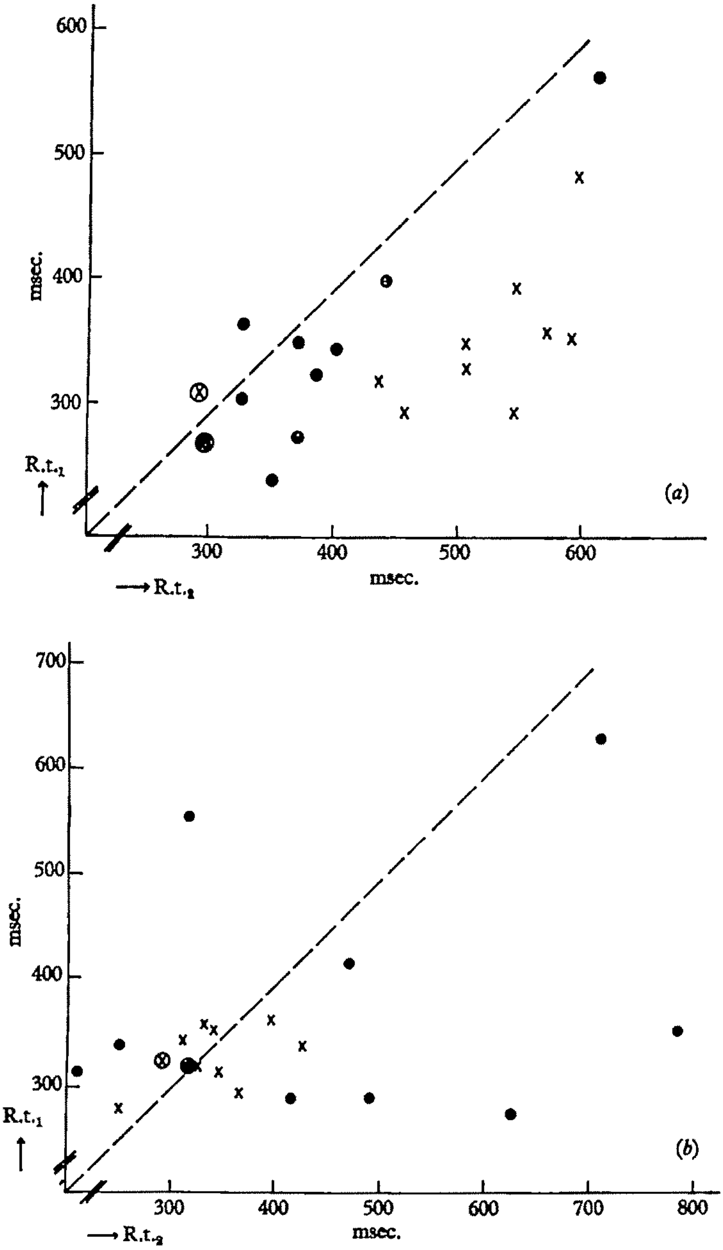

Fig. 1. (*a*) Right-handed subject. ×, L.O. R 100; ●, R.O. L 100; ⊗, mean single response L.O. R 100; ⊙, mean single response R.O. L 100. (*b*) Key as for (*a*).

several apparently independent responses when the stimuli were presented in the order left/right but the responses in the reverse direction were more variable and included several in which the second response was clearly delayed.

For the group as a whole, using the strict criterion given above, 36 (or 10 %) of the paired responses were classified as 'independent'.* Twenty-six of these occurred when the stimuli were presented in the order dominant/non-dominant and only 10 when the order was non-dominant/dominant ($P = 0{\cdot}01$, two-tailed). Of the remaining paired responses 121 were classified as delayed; of these 51 occurred when the order of presentation was dominant/non-dominant and 70 when the order was non-dominant/dominant ($P = 0{\cdot}10$, two-tailed). A 2 × 2 contingency table for both sets of observations, delayed and independent, gives a value for χ^2 of 8·9 ($P < 0{\cdot}01$) showing a significant degree of association between order of presentation and type of response.

The observation that 'independent' responses are organized more frequently when the dominant stimulus occurs first, raises the question as to whether the significantly higher over-all mean for all dominant responses occurring second—the apparent reversal of dominance—reflects not so much a slowing of all responses on the dominant side but a greater incidence of slow responses, since fewer independent responses with their associated faster times are made with the dominant hand second. This possibility can be checked by calculating the mean second responses for the independent and delayed responses separately. The analysis is given in Table 3.

Table 3

	n	Dominant (msec.)	n	Non-dominant (msec.)
'Independent' responses	10	283·5	26	292·4
'Delayed' responses	70	425·3	51	392·7

The independent responses show the same dominance relationship as the first and simultaneous responses, the reversed dominance effect observed for the second responses being a characteristic of the delayed responses. The mean difference observed, 32·54 msec., is statistically significant ($P < 0{\cdot}05$).

V. DISCUSSION

While there is some individual variation, group performance in the present experiment shows consistent dominance effects. Responses made by the dominant hand are, on the average, faster than those made by the non-dominant hand. This relationship holds for single responses, for paired responses to simultaneous stimuli, and for paired responses elicited by stimuli separated by 100 msec. provided there is no 'interference' between the responses. When responses to the second stimulus are delayed, however, the dominance effect is in the reverse direction, the faster response being recorded by the non-dominant hand.

A comparison of these results with other experimental findings is restricted by the lack of recent experimental data in this field. Earlier work is subject to the criticism that it may have been performed under less stringent conditions of experimental control and/or analysis. There have also been important differences in the task itself. The two-stimulus, two-response pattern of the present experiment does not seem to have been employed, a single stimulus, whether visual, auditory or tactile eliciting responses on both left and right sides. Direct comparison with our finding of reversed dominance in the second situation is therefore impossible.

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* In case it should be assumed that these were 'lucky guesses' it should be mentioned that the number of 'false' or double responses to single stimuli was only 6 out of 716 stimulus-response situations (0·08 %).

Where the literature does provide comparable data, the observation that single reaction times are faster on the average with the dominant hand has been widely confirmed and merits no further comment. There has, however, been considerable disagreement as to the dominance effects which occur when the two responses are required simultaneously. Such workers as Salow (1913), Buytendyk (1912) and Metfessel & Warren (1934) observed a reversal of dominance in this situation with the non-dominant side responding faster than the dominant. Piéron (1952) suggested that under this condition the non-dominant hand responds faster due to over-compensation. On the other hand, our observation that 'simultaneous' responses do not show this 'over-compensation' or dominance reversal is supported by the findings of Orton & Travis (1929), Terashi (1920) and Herlitzka (1908).

Although Herlitzka's results showed normal dominance effects, it was perhaps this author who was responsible for the concept of over-compensation. Herlitzka invoked the then-popular concepts of sensory and motor set to explain results contrary to those observed with his experimental group. He suggested that over-compensation would occur if a subject in attempting to correct a natural imbalance adopted a motor set which led to too great an inhibition of the faster response. This distinction between sensory and motor set could perhaps explain the over-compensation observed by other workers. Metfessel and Warren's subjects, for instance, were instructed to press both keys *simultaneously* in response to a visual signal.

Terashi considered the importance of set in relation to stimulus-response compatibility. The stimulus in his experiment consisted of a puff of air applied to the right forearm, the left forearm or the forehead. Dominance effects were consistently in the expected direction, with the dominant hand recording faster reaction times, but each hand responded faster with ipsilateral stimulation than when the stimulus was applied to the opposite hand. Frontal stimulation produced intermediate values.

With dominance effects similar to those of Herlitzka, Terashi was prepared to accept Herlitzka's postulate of a single gnostic centre localized in the dominant hemisphere, controlling the projection centres of both hemispheres. He noted, however, that without some additional hypothesis this would not explain his observation that responses were made relatively faster by the hand on the same side as the stimulus. This he explained in terms of the distribution of the subject's attention without suggesting any physiological mechanisms for the effect.

The concept that some physiological mechanism essential to the performance of the task may be located in only one hemisphere, has found many adherents from Herlitzka up to Craik (1948) and Welford (1952) in more recent times. It is difficult, however, to reconcile the dominance effects observed in the double stimulus/response situation in the present experiment with such a concept. The two observations which require theoretical explanation are (*a*) that two responses can be made apparently independently by some subjects and that the independence can be achieved more readily when the tasks are presented in one order (dominant/non-dominant) rather than the other, and (*b*) that where the response to the second stimulus is delayed—a much more common response pattern than independent responses—this delay is greater in the order non-dominant/dominant than in the opposite order. With regard to the delayed responses, reversal of dominance could only occur under a 'single channel' hypothesis if a perceptual or peripheral component showed a marked dominance effect. With the present group of subjects eye dominance was not consistently related to hand dominance and the effect seems rather large to be explained solely by differences in peripheral conduction time. Our second observation that two responses could be made independently suggests a more plastic mechanism than a 'single channel' one. One possibility is for a unilateral skilled response to be organized in the contralateral hemisphere. On this hypothesis the fact that independence of response is so rarely achieved implies that in organizing a first response some kind of contemporaneous activation of the opposite hemisphere occurs which interferes with the subsequent organization of a response in that hemisphere. The observation that independence can be more readily achieved when the stimuli are presented in the order dominant/non-dominant further suggests that the tendency of the non-dominant hemisphere to mirror, monitor or be otherwise stimulated by activity in the dominant hemisphere is less great than the converse relationship.

Unfortunately, this explanation appears to conflict with some recent observations made electromyographically. Cernaçek (1961), in a study of the inapparent activity in the symmetrical muscle of the opposite side of the body which may accompany a unilateral voluntary movement, found that irradiation occurred significantly more often from the dominant to the non-dominant hemisphere. An earlier experiment by Golla (1921) gave the opposite results, but as Golla examined only two subjects and his experimental conditions were markedly different, there is no reason to prefer his observations to those of Cernaçek.

Cernaçek quotes Hellebrandt's (1951) studies in cross-education in support of his findings, claiming that Hellebrandt showed that better results in the cross-education of a skill were obtained when the dominant extremity was exercised. Cernaçek's interpretation of Hellebrandt's findings is a fair one since the level of performance attained by the non-dominant hand when unpractised equals that attained by direct practice. Hellebrandt himself, however, uses a different criterion in judging the degree of cross-education. He says 'If the percentage gained by direct practice is compared with the improvement obtained concurrently by the unpractised contralateral limb the relative degree of cross-education appears to be somewhat greater from left to right than vice versa. The two hands achieve a more nearly equal mechanical ability than that attained during direct practice of the dominant extremity.' The data will bear either interpretation. Cook's (1934) very detailed investigation in cross-transfer of learning favours the interpretation of a greater degree of transfer from right to left, whichever criterion is adopted.

A much clearer result, however, was obtained in an early experiment on the division of attention. Welch (1898), using two tasks which could be performed concurrently by the two hands as in the present experiment, found a similar asymmetry of interference. 'From the study of the curves from the two hands I must conclude that the innervations to perform rhythmical contractions given to the left hand while the right hand is innervated to pull the dynamometer are communicated to the right hand and influence the form of the curve, while innervations to perform rhythmical contractions given to the right hand while the left is pulling the dynamometer are not transmitted or at least are transmitted much less markedly to the left hand.'

Although, therefore, there is some doubt about the direction of dominance effects when there is interference between the two hemispheres, there is general agreement that interference occurs in an asymmetrical way. Further, the findings of the present experiment support the contention that under appropriate conditions adequate pathways for the concurrent and independent performance of two simple skills can be demonstrated.

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Appendix I

The following tests or questions, mainly selected from Laterality Test Items used by Hull (1936), formed the basis of a sinistrality index for each subject:

1. Cross arms.
2. Cross hands.
3. Look through a small hole.
4. Hold finger in front of eyes.
5. Close easier eye.
6. Which hand do you use for writing?
7. Which hand do you use for eating (knife and fork)?
8. Which hand do you use for cleaning teeth?
9. Which hand do you use for playing games (tennis, cricket, etc.)?
10. Which hand do you use for dealing cards?
11. Which hand do you use for throwing?
12. Which foot do you use for kicking?
13. With which hand do you hold scissors?

The above items were scored as a maximum of 10 units, by combining the answers to 1 and 2, and to 3–5. Unequivocal answers of Left and Right were scored as 1 and 0 respectively; qualified responses were scored as 0·5. The answers to all items, totalled and divided by 10, yielded a sinistrality index for each subject.

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MEMORY SPAN AND THE SKEWNESS OF THE SERIAL-POSITION CURVE*

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Three experiments were performed to test the hypothesis that the skewness of the serial-position curve is determined, at least in part, by memory span. Expt. I showed that the degree of skewness of the serial-position curve is positively related to the length of the subject's immediate memory span. Memory span for sequences of colour-forms was determined for 47 subjects (university students). Eleven subjects with the highest span (HS) and 11 subjects with the lowest span (LS) were compared in the serial learning of a 9-item list of colour forms. The HS group produced significantly more skewed serial position curves both for overt errors and for failures to respond than did the LS group. Expt. II investigated the effects of prolonged practice on serial learning. Three subjects each learned a different 9-item list of colour-forms every day for 4 weeks (20 days). Practice increased the ease of serial learning to the point that the entire list could be comprehended within the subjects' memory span. There was a corresponding increase in the skewness of the serial-position curve as a function of amount of practice. Expt. III tested the hypothesis that a list of items for which subjects have a relatively long memory span would produce a more skewed serial-position curve than would a list composed of items for which subjects have a relatively short memory span. Forty subjects each learned a 12-item list of single letters (in a random order) and a 12-item list of 3-letter nonsense syllables. As predicted, the list composed of single letters produced a significantly more skewed serial-position curve than did the list of nonsense syllables.

I. INTRODUCTION

One of the problems of explaining the bow-shaped serial-position curve concerns the typical skewness of the curve. The skewness, with the maximum errors past the middle of the list, is greatest for short lists and decreases as the number of items in the list increases. The bow shape of very long lists (usually 16 or more items) appears almost symmetrical about the middle position (McGeoch & Irion, 1952, p. 123).

The first writer has proposed a theory of serial learning in which the skewness of the serial-position (SP) curve is related to immediate memory span (Jensen, 1962*a*). The aspects of this theory which are relevant to the present paper may be summarized briefly as follows: (*a*) When a series of items can be reproduced perfectly after one presentation, the number of items in the series defines the subject's memory span for these items under the particular conditions of presentation. When the number of items presented exceeds the subject's memory span, the additional items interfere with the retention of the items that fall within the subject's span and the subject recalls fewer items after a single presentation than if the list had not exceeded his memory span (Woodworth & Schlosberg, 1954, pp. 705–6). Also, the longer the list, the greater is the effect of this interference. (*b*) If the subject cannot learn the entire list within one trial, that is, if the list exceeds his memory span, he learns it piecemeal in *n* trials. Consequently some items are learned sooner than others. (*c*) The SP curve results from the fact that the units of the serial list are learned in a fairly definite order as a function of serial position. The determinants of this order are as yet not fully understood, but it is hypothesized that the units of the list are built-up,

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integrated, or 'attached' around an anchor point, which is usually the first item or items learned on the first presentation of the list. On successive trials new items are attached to the anchor point in a forward and backward direction, so that, for example, the items of a 9-item series would be learned as follows:

Position of item: 6 7 8 9 *1* 2 3 4 5
Order of learning: 9 7 5 3 1 2 4 6 8

or

Position of item: *1* 2 3 4 5 6 7 8 9
Order of learning: 1 2 4 6 8 9 7 5 3

This simple model, which is easily generalized to lists of any length, predicts quite well the peak of the SP curve for errors and the degree of skewness of lists of various lengths (Jensen,1962*a*). The errors at each position of the SP curve are directly proportional to the rank order of learning. (*d*) It is hypothesized that the skewness of the SP curve for a list of a given length is determined in part by the subject's memory span for the type of material composing the list and by the conditions of presentation of the list. The longer the subject's memory span, the more items that are learned in the forward direction from the anchor point. Thus, a subject with a very short memory span would learn a 9-item list as follows:

Position of items: *1* 2 3 4 5 6 7 8 9
Order of learning: 1 3 5 7 9 8 6 4 2

A subject with a long memory span, on the other hand, would learn the same series as follows:

Position of items: *1* 2 3 4 5 6 7 8 9
Order of learning: 1 2 3 5 7 9 8 6 4

Since the theory states that the errors (overt errors plus omissions) made at each position before the entire list is mastered are directly proportional to the rank order in which the items are learned, it can be seen from these two examples that the curve corresponding to the longer memory span is the more skewed; that is, when the memory span is longer the last positions are learned relatively later and a greater proportion of the total errors occur in the last half of the list.

The present experiments attempt to test this aspect of the theory which relates skewness of the SP curve to memory span. Other aspects of the general theory of serial learning (see also Jensen, 1963) are not discussed here, since they are not directly relevant to the memory span hypothesis.

This hypothesized relationship between memory span and serial learning is not new. Lepley (1934, ch. v) found that when subjects learned twelve different lists, the only systematic effect that practice had on the shape of the SP curve was to produce greater skewness. With an increasing amount of practice, the peak of errors moved further toward the end of the list, and the percentage of total errors in the first half of the list decreased. The most pronounced effect of practice was, of course, to increase the over-all speed of learning, but this increase was greatest in the first part of the list, with the consequent increase in skewness of the SP curve. This increased skewing might be explained in terms of the memory span hypothesis, since it has also been shown that memory span is increased by practice (Gates & Taylor, 1925).

Lepley found also a difference between low I.Q. 7th-grade boys and high I.Q. 11th-grade boys in serial learning (Lepley, 1934, ch. VI). Not only were the older, superior boys (mean mental age = 19 yr. 2 mo.) faster learners than the younger boys with low I.Q.'s (mental age = 12 yr. 5 mo.), but the SP curve of the superior group was significantly more skewed than that of the low I.Q. group. Since the two groups very likely differed in memory span (Digit Span norms for the Wechsler Intelligence Scale would indicate a difference of at least 2 to 3 digits (Wechsler, 1944, p. 222), this finding also lends support to the memory span hypothesis. The same phenomenon was reported by Barnett, Ellis & Pryer (1960), who compared the SP curve produced by adolescents with high I.Q.'s (120–139) with the curve produced by adolescents with low I.Q.'s (40–59). The curve of the high I.Q. group was significantly more skewed than that of the low I.Q. group.

Since memory span is known to be correlated with mental age, our hypothesis would also predict that the skewness of the SP curve should increase as a function of age among children of normal intelligence, with the increase in skewness becoming asymptotic at about age 18 or 19, the age beyond which memory span does not increase appreciably. This prediction has been borne out in an experiment by Postman & Jensen (1963), in which groups of normal, 4th-, 7th-, and 10th-grade children and of university sophomores, with mean ages of approximately 9, 12 15 and 19, respectively, learned 12-item lists consisting either of words or of nonsense syllables. As predicted, the SP curves of the four age-groups yielded significant differences in skewness, both for words and for nonsense syllables. A quantitative index of skewness was shown to increase as an approximately linear function of age.

Malmo & Amsel (1948) compared the SP curves of patients suffering functional and organic memory loss (severe and bilateral frontal gyrectomy) and those of normal subjects. The SP curves of the patients were more symmetrical about the middle position than was the curve for the normal subjects, and the patients made significantly more errors in the first half of the list. Though Malmo & Amsel interpreted their results in terms of anxiety-produced interference, their findings are clearly in accord with the memory span hypothesis, since anxiety-produced interference also adversely affects the subject's memory span (Siegman, 1956).

Deese & Kresse (1952) found that the SP curves for failures-to-respond and for overt errors differ in shape. The failures rise to a maximum at about the middle of a 12-item list and show very little decrease in the positions beyond the middle. The overt errors, on the other hand, form a bow-shaped curve almost perfectly symmetrical about the middle position. The composite of the two curves forms the typical skewed, bow-shaped SP curve for total errors. To explain these findings Deese & Kresse suggested a two-factor theory. 'A factor of associative inhibition would be indicated by the intra-list intrusions [overt errors], and a second factor, that might be identified with either the notion of response-induced inhibition or the *immediate memory span* [our italics], would be indicated by the failure-to-respond curves' (1952, p. 202).

Since the experiments we have reviewed did not include the independent measurement of the memory span and are therefore merely suggestive of our hypothesis, the present experiments were conducted specifically to investigate the role of memory span in serial learning. The relationship between individual differences in memory span and the degree of skewness of the SP curve was examined in Expt. I. Expt. II was concerned with the effect that prolonged practice in serial learning has on the skewness of the SP curve. It was presumed that practice would increase memory span, at least for the kind of items being practised, and that, in accordance with our hypothesis, the increase in memory span would influence the skewness of the SP curve. Expt. III tested the hypothesis that materials for which subjects have a relatively long memory span, when learned as a serial list, would produce a more skewed SP curve than would materials for which subjects have a relatively short memory span.

II. EXPERIMENT I

Method

Subjects. A class of 47 college juniors and seniors in an introductory educational psychology course served as subjects.

Memory span test. In order to minimize the verbal associations that arise in using nonsense syllables, meaningful verbal materials, or digits, the content of the memory span test consisted of geometric shapes in various colours, viz. squares, triangles and circles, coloured red, yellow and blue. Thus there were three shapes in each of three colours, making nine different stimuli in all. The serial learning task consisted of the same set of stimuli, since it was desired to measure memory span and serial learning in the same medium.

The subjects were instructed that they would be shown a sequence of stimuli on the screen at the front of the room. The stimuli were 4 in. in size on the screen and were of vivid colour. There were from 3 to 7 stimuli in each sequence, but the number appearing in any sequence was determined randomly and was thus unpredictable by the subject. However, the same length of sequence was never presented twice in succession. The stimuli were presented one at a time at a 2 sec. rate. The subjects were told the nature of the stimuli as well as the fact that the same shape or the same colour would never appear consecutively. Subjects wrote their responses in the form of initial letters (e.g. BC for blue circle). Guessing was allowed. After all subjects had recorded their responses, the next sequence was presented. Each sequence was a different random order. In all, there were three presentations each of sequences having either 3, 4, 5, 6, or 7 stimuli. The subjects sat one seat apart and were proctored to make sure they did not record their responses until after the sequence had appeared.

Serial learning. The 11 subjects with the highest memory span and the 11 subjects with the lowest span were tested individually in the serial learning task. The subjects were instructed to learn by the anticipation method a series of 9 stimuli presented at a 3 sec. rate. The same sequence was repeated, with a 9 sec. intertrial interval, until the subject correctly anticipated all nine items in the series. Subjects were told that the stimuli were exactly the same as those used in the memory span test; the same rule held that the same shape or colour would never appear adjacently in the series. The stimuli appeared on a screen 12 ft. in front of the subject. The series was always preceded by three small dots as the signal to anticipate the first item. The subject responded by saying 'blue triangle', etc. Three different serial orders were used, with each group of 11 subjects divided 4, 4, 3.

Results

Memory span. Memory span was defined as the longest series the subject could recall perfectly after a single presentation. Since there were three sets each of 3, 4, 5, 6, and 7 items, the subject's memory span score was the mean of the three longest lists he recalled perfectly. The reliability of this score, as determined from the intraclass correlation among the three sets, was 0·57. The mean span for the entire group ($N = 47$) was 2·51, S.D. 1·47. From this group were selected the 11 subjects with the lowest memory span score and the 11 subjects with the highest. The mean span for the Low Span Group (LSG) was 0·55, S.D. 0·50; for the High Span Group (HSG) the mean span was 4·48, S.D. 0·41. The difference between the means of the LSG and HSG is reliable ($t = 19{\cdot}22$, $P < 0{\cdot}001$).

Serial learning. The HSG required fewer trials to attain the criterion of mastery than did the LSG, though the difference is not statistically significant (Table 1*a*).

Table 1. *Means of high and low memory span groups on serial learning variables*

Variable	High span		Low span		
	M	S.D.	*M*	S.D.	*t*
(*a*) Trials to criterion	13·5	5·5	18·1	6·8	1·64
(*b*) Percentage of overt errors in first 4 positions	37·8	9·4	46·8	13·5	1·72*
(*c*) Percentage of failures in first 4 positions	14·3	8·8	23·8	9·1	2·33**
(*d*) Percentage of all errors that are failures-to-respond	47·7	25·3	58·3	13·9	1·16

*$P < 0{\cdot}10$. **$P < 0{\cdot}05$.

Fig. 1 shows the mean number of trials to attain successive numbers of correct anticipation per trial. The left side of the figure shows the rates of learning the first four positions in the series (positions 1–4); the right side shows the rates of learning the last four positions (positions 6–9). The data of these curves were subjected to an analysis of variance, shown in Table 2. It can be seen that the HSG learned the first part of the list significantly faster than the LSG and that the two groups differ hardly at all in the rate of learning the last part of the list. This difference in rate of learning the two parts of the list is reflected in the skewed appearance of the SP curve. The difference between the HSG and LSG shows up even in the first learning trial, though not to a statistically reliable degree, since the within-groups variability is quite large on single-trial data. But the first-trial data are wholly consistent with the over-all results. For example, on the first trial the HSG had a total of 30 correct anticipations, of which 19, or 63 %, occurred in the first four positions, while the LSG produced a total of 25 correct anticipations, of which only 13, or 52 %, were in the first four positions. The peak of errors on the first trial was at position 4 for the LSG and at position 6 for the HSG. On the first trial the total number of anticipations *consecutively* correct from the beginning of the series was 15, or 50 % of all correct anticipations, for the HSG; LSG had 10 (40 % of all correct responses) consecutively correct anticipations.

The SP curve for *overt* errors, all of which were intralist intrusions, is shown in Fig. 2. Since we were interested primarily in the shape of the SP curve, the curves in Figs. 2 and 3 were obtained by determining the percentage of errors (or failures) made at each position by each subject and then obtaining the mean for all subjects in each group. Thereby every subject is weighted equally in the mean curve and the areas under the curves are the same for both groups. The only possible difference is in the shape of the curve. The percentage of all overt errors occurring in the first four positions provides a rough index of the degree of skewness. The HSG and LSG differ in this measure (Table 1*b*).

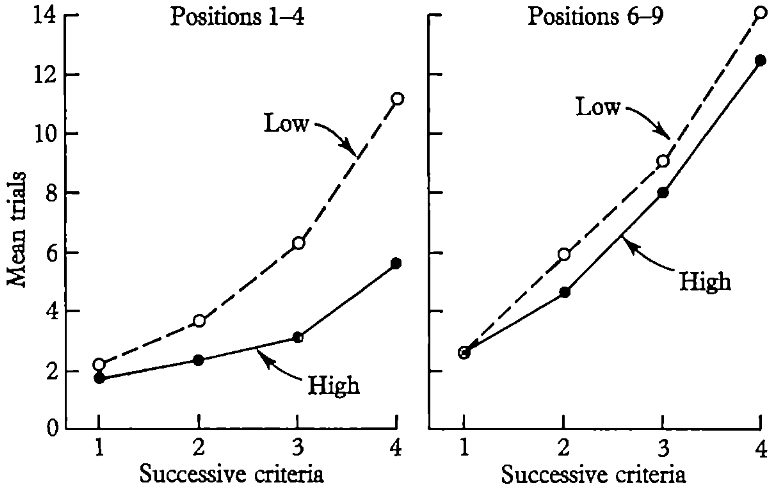

Fig. 1. Mean number of trials to attain successive numbers of correct anticipations per trial for the first 4 serial positions (positions 1–4) and for the last 4 positions (positions 6–9).

Table 2. *Analysis of variance of trials to successive criteria for positions* 1–4 *and positions* 6–9

		Positions 1–4		Positions 6–9	
Source	D.F.	M.S.	*F*	M.S.	*F*
Memory span (MS)	1	147·73	7·52*	0·56	< 1
Criteria (C)	3	175·79	—	467·80	—
Subjects within groups	20	19·64	—	41·99	—
MS × C	3	27·21	6·21**	9·53	1·06
Error	60	4·38	—	8·98	—

*$P < 0·05$. **$P < 0·001$.

Fig. 3 shows the SP curves of mean percentage of failures-to-respond. As was the case in the Deese & Kresse (1952) study, the curves for failures-to-respond are much more skewed than those for overt errors, and this is true for both the HSG and LSG. However, the skewness was again greater for the HSG. There is a significant difference between the groups in the percentage of all failures that occur in the first four positions (Table 1*c*). The greater skewness of the SP curves of the HSG is not attributable to the HSG having a larger percentage of failures among its total errors, for, in fact, the LSG had a larger number of failures in proportion to overt errors than did the HSG, although this difference is not statistically significant (see Table 1*d*).

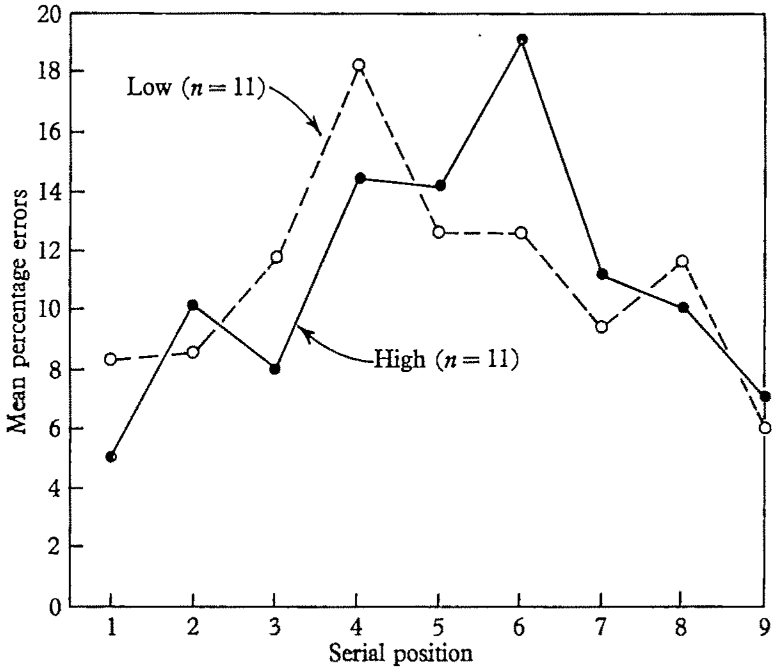

Fig. 2. Serial-position curves showing the mean percentage of overt errors at each serial position for subjects with high or low memory span.

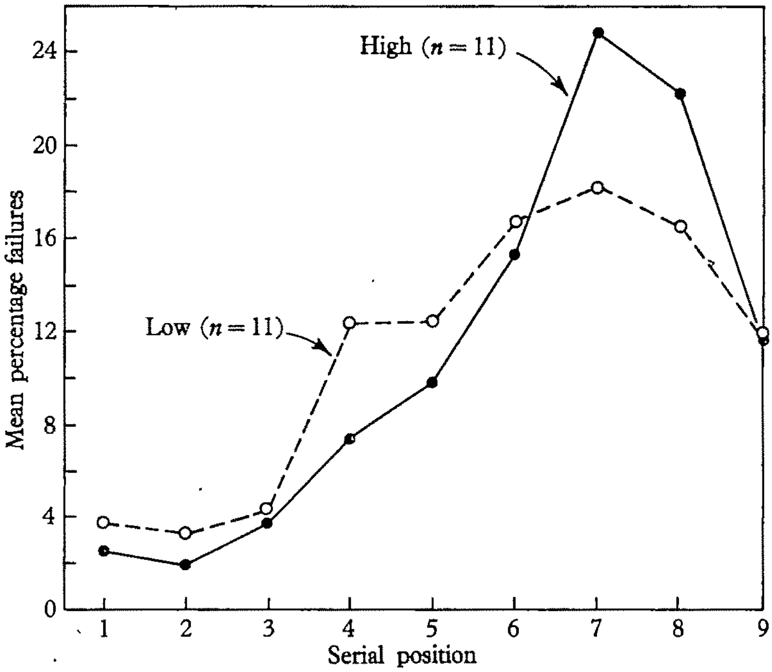

Fig. 3. Serial-position curves showing the mean percentage of failures-to-respond at each serial position for subjects with high or low memory spans.

III. EXPERIMENT II

Method

Subjects. Three women students (one senior and two graduates), ages 20, 21, and 22 volunteered as subjects.* As was later discovered, they were not the ideal subjects for the purposes of this experiment, since they all were exceptionally fast learners and started out with highly skewed SP curves. Consequently there was little room for additional practice in serial learning to make much difference. All three subjects were above the 95th percentile on the American Council on Education Psychological Examination, a test of scholastic aptitude. Their only knowledge concerning the purpose of the experiment was that we were investigating practice effects in serial learning.

Procedure. The subjects learned, by the anticipation method, a different serial order of nine coloured forms each day of the week (i.e. 5 days) for 4 consecutive weeks. Thus there were 20 days of practice in all. The serial learning materials and procedure were the same as in Expt. I. The general instructions were given to the subjects only the first day and on each succeeding day they were merely reminded that their task was the same as the day before except that they had to learn a new order of the stimulus items. Twenty random orders of the stimuli were used, with the one restriction that the same shape or the same colour would never appear adjacently in the series. The order in which the lists were given to the subjects each day was random for each subject. The subjects always attained a criterion of one perfect trial. They were required to guess when in doubt, rather than omit response. In order to get information about improvement in their guessing ability, the subjects were required to anticipate each stimulus item on the very first presentation. (These first-trial guessing data were not included in the analyses of the serial learning or in the SP curves.)

Results

Rate of learning. The most obvious effect of practice was to increase the rate of learning. The mean trials-to-criterion on the first day's serial list was 7·67, which is about 1 S.D. below the mean of college students learning these kinds of lists in previous experiments. The mean trials-to-criterion during each of the 4 weeks were 4·9, 2·7, 2·5, and 2·1, respectively. This decrease in mean trials-to-criterion is significant beyond the 0·01 level ($F = 16{\cdot}74$, D.F. = 3, 6). The over-all decrease in total errors from the first 2 weeks to the second 2 weeks of practice is evident in Fig. 4. There were also significant individual differences in learning rate. The mean trials to criterion for each of the three subjects were 3·9, 3·0 and 2·4. The differences between these subjects' means are significant beyond the 0·05 level ($F = 7{\cdot}95$, D.F. 2, 6).

That memory span increased with practice may be inferred from the fact that by the final week all subjects were able, part of the time, to recall the entire list after a single presentation. It appears that after 3 to 4 weeks of practice the subjects became able to comprehend the 9-item series within their immediate memory span. In the fourth week, for example, one subject recalled perfectly one of the lists, and two subjects each recalled three lists, after a single presentation. This one-trial learning never occurred during the first week of practice and occurred only once in the second week.

Serial-position effect. Our chief concern, of course, is the effect of practice on the SP curve. To obtain fairly reliable curves, the data of weeks 1 and 2 (days 1–10) were combined for comparison with the combined data of weeks 3 and 4 (days 11–20).

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* We are grateful to Joanne Clason, Arlene Rustin, and Ida Yamamoto for so faithfully and uncomplainingly submitting to this ordeal.

These SP curves, expressed in terms of the total number of errors in each position, are shown in Fig. 4. (Because of the negligible number of failures-to-respond among the three subjects in Expt. II, we have not distinguished between failures and overt errors but have simply labelled them all as *errors*.) In general, after 2 weeks of practice, the first 3 or 4 positions appear almost equal in difficulty, and the peak of errors has moved one position toward the end of the series. To obtain a quantitative measure of the degrees of skewness, we have used, as in Expt. I, the percentage of all errors that occur in positions 1–4. Degree of skewness is, of course, inversely related to this percentage. The mean percentages of errors in positions 1–4 in each of the four weeks of practice are shown in Fig. 5. The geometric mean (G.M.) of the percentages is presented in addition to the arithmetic mean.* Since the over-all errors decreased markedly with practice, the percentage of errors in positions 1–4 tends to fluctuate considerably from day to day and in the last 2 weeks their distribution was very positively skewed. In such a case, the G.M. would seem better to represent the trend of the data. The percentage data shown in Fig. 5 were subjected to analysis of variance, which required that the percentages be transformed to log $(x+1)$ to achieve homogeneity of variance from week to week. The between weeks main effect, tested against the weeks × subjects interaction, is significant beyond the 0·05 level ($F = 5{\cdot}84$, D.F. = 3, 6). Reliable individual differences in the degree of skewness of the SP curve are indicated by the significant between subjects effect ($F = 7{\cdot}64$, D.F. = 2, 6, $P < 0{\cdot}05$).

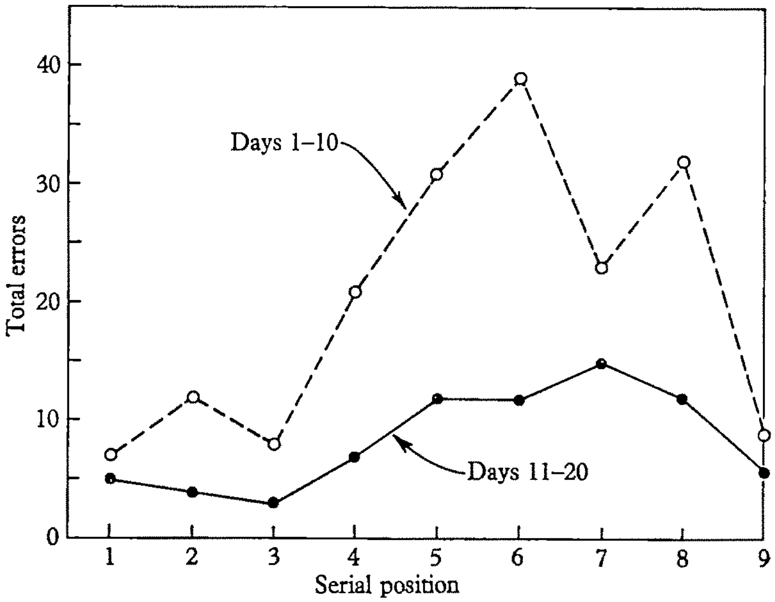

Fig. 4. Serial-position curves of total errors made during the first two weeks of practice (days 1–10) and during the second 2 weeks (days 11–20).

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* Since zero cannot enter into the computation of the G.M. 1·00 was added to all the original percentages.

Guessing tendencies. Inspection of the first-trial data, in which every response was a sheer guess, indicates that changes in the subjects' guessing tendencies over the 4 weeks probably worked against our hypothesis, viz. that, with increasing practice, the preponderance of errors would gravitate more and more to the last half of the list.

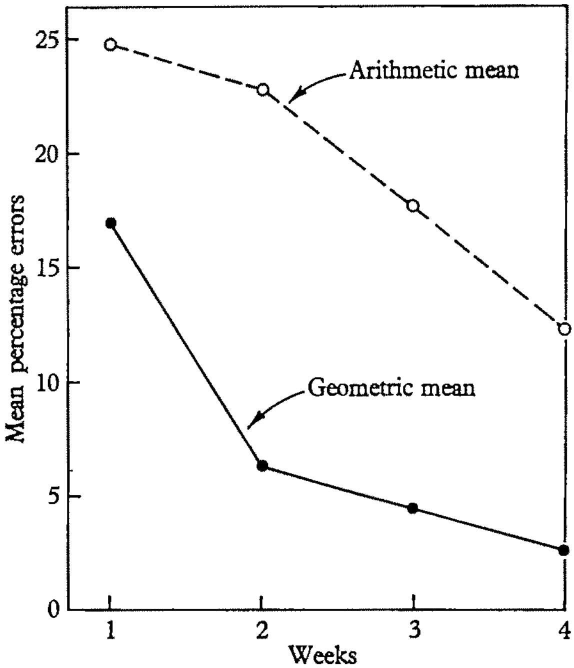

Fig. 5. Mean (arithmetic and geometric) percentage of total errors occurring in serial positions 1–4 (of a 9-item list) as a function of weeks of practice.

There was a small, non-significant improvement in guessing over the weeks. The mean number of correctly guessed anticipations during the first 2 weeks was 3·9; during the second two weeks it was 4·3. Of greater importance in terms of our hypothesis, however, is the fact, that, with practice, the subjects improved in guessing correctly the last two or three items of the list. By the time they were halfway through the list they seemed to remember which items had appeared previously and did not use them in making their guesses. The effect, of course, was to make the guessing probabilities of the last items considerably higher than those in the first half of the list. In the first-trial guessing during the first 2 weeks, 59% of all errors occurred in positions 1–4; during the second 2 weeks 62% of the errors occurred in positions 1–4. If it is assumed that the subjects' guessing strategy carried over the learning trials, this would seem to have worked against our hypothesis that practice would result in a decrease in percentage of errors in positions 1–4. The hypothesis was borne out, however, despite this counteractive effect of the subjects' guessing tendencies.

IV. EXPERIMENT III

While the previous experiments show that individual differences in memory span as well as the improvement of ability in serial learning as a result of prolonged practice affect the skewness of the SP curve, a further question remains: Do different materials for which subjects in general have longer or shorter memory spans produce SP curves that differ in skewness? This question cannot be satisfactorily answered by the inspection of the SP curves found in the literature on serial learning, mainly for four reasons: (*a*) subjects across various studies are not of comparable learning ability; (*b*) the conditions of learning, such as instructions, presentation rate, distribution of practice, etc., are not comparable across various studies; (*c*) little or nothing is known concerning subjects' memory span for the different materials generally used in serial learning experiments; and (*d*) SP curves as generally plotted confound a number of factors, making the comparison of different curves quite ambiguous with respect to our present hypothesis (see Jensen, 1962*b*). It was necessary, therefore, to carry out an experiment specifically to determine whether or not different material for which subjects have a relatively long memory span produce a more skewed SP curve than do materials for which the memory span is relatively short.

Method

Subjects. Forty students in an introductory course in educational psychology served as subjects.

Procedure. Each subject was required to learn two serial lists of 12 items each. The criterion was one errorless trial. One list (for long memory span) was composed of single letters (A to L); the other list (for short memory span) was composed of 3-letter trigrams (*bot, com, dal, est, fac, mul, nop, pim, rel, tis ven, zin*). To average out any specific interaction between items and positions, the items were presented in a different random order for every one of the 40 subjects. Half the subjects learned the letter list first and half learned the trigram list first. Subjects were tested individually and were given the standard instructions for learning by the anticipation method; they pronounced the syllables. The items were automatically projected on a screen at a 3 sec. rate with a 6 sec. intertrial interval. (The apparatus used in these experiments is described in detail elsewhere (Jensen, Collins & Vreeland, 1962). The first item of the list was always preceded by a green light of 3 sec. duration which served as the signal to anticipate the first item.

Results

The number of errors (overt errors plus omissions) in the first half of the list (positions 1–6) and in the second half of the list (positions 7–12) were determined for each subject for the letter list and for the trigram list. These data were subjected to an analysis of variance. The over-all skewness effect was highly significant, with a greater number of errors in the second half of the series than in the first half ($F = 36{\cdot}82$, D.F. = 1 and 39, $P < 0{\cdot}001$). Of course there was a large difference in the over-all difficulty of the two lists, the mean trials for letters and for trigrams being 4·63 and 11·29, respectively.

The test of the memory span hypothesis, however, is the lists × halves interaction, which proved significant beyond the 0·01 level ($F = 8{\cdot}17$, D.F. = 1 and 39). The letter list produced a significantly more skewed SP curve than did the trigram list. The percentage of the total errors in the first half of the list (positions 1–6) was 34·4 for letters and 37·3 for trigrams. Though the absolute difference does not seem great, it is fully significant as shown by the analysis of variance. Also in accord with the memory span hypothesis is the fact that the mean number of consecutively correct responses from the beginning of the list on trial 1 (i.e. the first anticipation trial) was 3·34 for the list of letters and 1·54 for the trigrams. The correlated t test (by the direct difference method) shows these means to be significantly different beyond the 0·001 level ($t = 4{\cdot}12$, D.F. $= 39$).

V. DISCUSSION

In Expt. I a relationship was found between individual differences in memory span and the degree of skewness of the SP curve. Expt. II, in which subjects practised serial learning until the entire list could be comprehended by the immediate memory span, revealed a progressive increase in skewness as a function of practice. The latter finding was originally made by Lepley (1934, ch. v), using 12-items lists of nonsense syllables. Since Lepley used different syllables in each of the 12 lists learned by his subjects, the practice effect on the serial learning *per se* was probably somewhat obscured by the subjects' having to acquire the responses anew on each successive list. In the present experiment, on the other hand, all the learning was limited to serial order. It is known from previous experiments with our colour-form stimuli that subjects are able to name all the items comprising the list even before the first presentation, since the nature of the items is described in the experimenter's instructions to the subject. Under these conditions, however, the subjects' guessing strategies may have obscured to some extent the hypothesized relationship between span and skewness.

Expt. III showed that skewness is not only a function of individual differences in memory span for a given material but also of differences in memory span for different kinds of material composing the serial list.

Probably a greater degree of relationship between skewness and individual differences in memory span, and between skewness and amount of practice, would have been obtained had we selected subjects from a more heterogeneous population than our upper-division college group. Both our High span and Low span groups, for example, probably produced quite skewed SP curves as compared with what might be found in a less select and less fast-learning population. Mentally retarded subjects with a very small memory span, if they can learn a serial list at all, might be expected to produce SP curves having almost no skewness.

Also, a short list of, say, 6 or 7 items, might reveal a greater association between memory span and skewness, since as the number of items in the serial list increasingly exceeds the subject's memory span, there is probably a corresponding increase in retroactive inhibition of the first part of the list. If there were a negligible correlation between memory span and susceptibility to the effects of retroactive inhibition, lists that greatly exceed the memory span would tend to obscure the effects of memory span in determining the shape of the SP curve. In very long lists of, say, 14 or more items, we would expect individual differences in memory span to be reflected only slightly in the shape of the SP curve. The soundness of such a prediction depends, of course, upon the nature and degree of relationship between the subject's memory span and the degree of decrement in the subject's span which results, presumably from interference, when items are presented in excess of the subject's maximal span.

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EASE OF CONDITIONING AND SPONTANEOUS RECOVERY FROM EXPERIMENTAL EXTINCTION*

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Thirty adult male volunteers were conditioned and extinguished in a classical eyeblink conditioning situation to the same criterion and tested 24 hours later for the pattern of spontaneous recovery (reminiscence) and re-extinction. As predicted in terms of the decay of inhibition explanation of reminiscence phenomena, those subjects who required most reinforcements to attain the criterion of conditioning and least repetition of the conditioned stimulus to attain the criterion of extinction were also the subjects who showed greatest reminiscence. Certain limitations and possible applications of these findings are discussed.

I. INTRODUCTION

Spontaneous recovery of classical conditioned responses was originally described by Pavlov (1927). More specifically, it has been well established that the eyelid conditioned response, usually to light, exhibits a high degree of spontaneous recovery after a 24 hr. interval (Porter, 1938, 1939; Grant, Hunter & Patel, 1958; Howat & Grant, 1958; Prokasy, 1958; Hartman & Grant, 1960; Beeman, Hartman & Grant, 1960; Froseth & Grant, 1961; Beeman & Grant, 1961). Discussion of such phenomena has proceeded along four directions: empirical considerations, Hullian I_R theory, Spence's drive theory and Estes's statistical model.

Razran (1939) examined the 48 hr. spontaneous recovery of the salivary conditioned response to light, and concluded that the amount of recovery is directly proportional to the amount of extinction, and, if the extinction is only partial, to the degree of forgetting of the non-extinguished proportion of the conditioned response. His re-analysis of partially comparable data obtained by Hovland (1937) confirmed this conclusion. According to Hullian theory (Hull, 1943, pp. 295 ff., 391 ff.) an adequate rest pause after a period of continual or continuous work ought to permit complete or partial dissipation of reactive inhibition (I_R) accumulated during the preceding work period. This explanation, more usually given to account for the phenomenon of reminiscence in motor tasks, may be used to account for the spontaneous re-emergence of a conditioned response 24 hr. after apparent total extinction. Spence's drive theory, at least according to Beeman & Grant (1961), might lead to an expectation of an even greater reduction of unconditioned stimulus induced drive (D) during the delay period than during the immediate post reinforcement extinction period. Hence, rather than predicting any spontaneous recovery, the effect would, if anything, be in the opposite direction, namely to exacerbate the extinction effect. Estes's model (1955) would lead to a similar prediction.

A Hullian explanation of spontaneous recovery presents a number of formidable theoretical and practical problems (see, for example, Kimble, 1961, pp. 284 ff.) which do not seem soluble by the mere manipulation of already defective variables in preference to the postulation, quantification and experimental investigation of new ones. Eysenck (1955), discarding Hull's peripheral components of I_R, has postulated that human beings differ with respect to the strength of I_R produced and to the speed with which it is produced and dissipated.* This modified Hullian construct might perhaps be better designated as *reactive central inhibition* (I_{RC}). As Becker (1960), among others, has pointed out, even this use of the term inhibition lends itself to possible confusion, being used sometimes to represent a basal or constitutional property of nervous tissue and sometimes to denote a more ephemeral reaction of such tissue to stimulation. The two notions are not incompatible and may even be closely related.

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* Based in part upon a paper presented at the Annual Meeting of the British Psychological Society Reading, April 1963.

The phenomenon of spontaneous recovery of an extinguished conditioned response after an appropriate rest pause may be regarded as an example of reminiscence in which I_{RC} is generated during the acquisition and extinction 'practice' periods and permitted to decay during the subsequent 24 hr rest pause. For I_{RC} to induce maximum reminiscence in a manner that can be effectively detected and measured it is necessary that the various work, rest, and measurement procedures and units be precisely delineated. Often this is theoretically and practically difficult, and sometimes impossible. Thus, if the immediate post-rest unit used to calculate the reminiscence score is too long, the effects of post-rest extinction may swamp those of reminiscence; if the post-rest unit is too short, problems of reliability of measurement arise. If the practice period is too long, conditioned inhibition will be formed and conditioned inhibition is believed not to dissipate with rest. If the rest-period is too short, only part of the accumulated I_{RC} will have dissipated. If the pre-rest massed practice period is not sufficiently massed, too much inhibition will decay during this period and not leave enough for readily detectable decay during the rest period. Considerations such as these make both the measurement of spontaneous conditioned eyeblink recovery and its interpretation in terms of I_{RC} a complicated matter.

There is evidence (e.g. Franks, 1960) to suggest that the construct of I_{RC}, used as either a basal or a more truly reactive concept, may be successfully employed to account for individual differences in classical eyeblink conditioning, rapid and strong presence or generation of I_{RC} being associated with slow and weak conditioning and little resistance to extinction. It might further be expected that, given appropriate test procedures, strong I_{RC} would also be associated with much spontaneous recovery (reminiscence), together with a relatively rapid decay of this recovery. This rapid decay could be brought about both by inhibition occurring as a reaction to the production of the recovered conditioned responses during this recovery period and by the presence of a more enduring and constitutional inhibitory state. Conversely subjects who form conditioned responses readily might be expected to extinguish these responses slowly, and to manifest relatively little spontaneous recovery. This recovery, however, should persist relatively longer than that of subjects who condition poorly and show much reminiscence. Thus the two recovery curves of the good and poor conditioners might be expected to cross over each other.

Response frequency in such situations should be negatively related to I_{RC} and hence positively related to inter-trial interval. Although a more recent report of

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* Two years later, it should be noted, Eysenck (1957, p. 114) introduced another conflicting component when he further posited individual differences in rates of production and strength of *excitation*.

Prokasy & Whaley (1961) casts some doubt on these relationships, three earlier studies (Baron, 1952; Spence & Norris, 1950; Prokasy, Grant & Myers, 1958) have uniformally reported positive relationships between response frequency during eyeblink conditioning and inter-trial interval. Therefore, to generate maximum I_{RC} and give it minimum time to dissipate prior to the 24 hr. rest period, the inter-trial interval should be short during the acquisition and extinction phases.

II. EXPERIMENT*

In a newly constructed sound-proof laboratory 35 male adult volunteers, free of any known C.N.S. impairment and not currently receiving any drugs known to have central effects (including tea, coffee, or alcohol for at least 3 hr. prior to both testing and retesting), were subjected to an eyeblink conditioning acquisition and extinction procedure which involved the presentation of paired tone and air-puff stimuli in a partial reinforcement sequence. One or more non-reinforced test trials were interspersed at standard (although irregular) intervals among the reinforced trials until subjects produced conditioned responses to three successive test trials. When this criterion of conditioning had been attained the response was immediately extinguished by successive non-reinforced test trials, until a criterion of three successive failures to yield a conditioned response was attained. Twenty-four hours later all subjects were tested for spontaneous recovery and re-extinction by presenting the non-reinforced test stimuli 20 times in succession. (In a pilot run 20 test trials had been judged sufficient to extinguish the responses of most subjects at least to the criterion of three successive failures.

Prior to conditioning an estimate of spontaneous blink rate was obtained while subjects gazed for 3 min. at a small picture hanging on a wall approximately 6 ft. away, the middle minute being scored for frequency of blinks. Approximate binaural earphone hearing thresholds and air-puff sensitivity thresholds in the right eye were then determined, following which it was explained that 'we are interested in knowing how well you can relax when you receive puffs of air in the eye or tones over the earphones while you are resting and looking at this picture'. Each subject then received five conditioned stimulus tone trials separated by irregular intervals of 15 to 30 sec. and delivered to both ears through a pair of balanced earphones. This test of initial sensitivity to sound was followed immediately by five unpaired unconditioned stimulus corneal air puffs delivered to the right eye at similar irregular intervals. The procedure involving five tones alone was then repeated to provide a crude measure of pseudo-conditioning. Subjects were rejected if they responded by eyeblinks to more than one of either the first or last five tone trials. The conditioning tone, at a frequency of a 1000 cyc./sec, lasted 850 msec. and had an intensity of 60 db. The unconditioned stimulus, at a pressure of 170 mm. of mercury, lasted for 450 msec. For reinforced trials the tone preceded the air puff by 400 msec. The intertrial intervals ranged between 10 and 15 sec. during the acquisition phase and 7 and 12 sec. during the extinction and re-extinction phases. The criterion of conditioning was three successive responses to the test trial alone, a conditioned response being defined as any eyeblink having an amplitude equal to or greater than one-quarter the mean amplitude to the five air-puffs given in the pseudo-conditioning test and a latency period between 156 and 625 msec. from the onset of the conditioned stimulus.

Apparatus

The tone-air-puff conditioning system consisted essentially of an audio-oscillator wired to a pair of balanced earphones, an adjustable air-pressure source and a photoelectric eyeblink recording system evolved out of an earlier technique (Franks & Withers, 1955). A pair of plain lucite spectacles had a small infrared photoelectric cell mounted above them so that a constant distance was maintained from the eye regardless of head position. One lens contained several small apertures for housing the air-puff delivery tube in whatever position was most likely to facilitate a blink response. The light source was a 1·5 V. flashlight bulb mounted so that its tip protruded into a hole drilled through the right lens. Black paint covered all but the tip of this bulb, confining radiation to a narrow beam directed toward the eye. A small piece of Kodak infrared filter cemented over the projection area on the inside lens surface reduced visible illumination to a barely noticeable glow. Fluctuations in the photocell resistance, produced by reflexion variations in the accompanying eyeblinks, were fed to an a.c. amplifier coupled to one channel of the recording galvanometer. Pressure was supplied by a continuously running air compressor fed into a 200 cu.in. storage tank. Air was conducted from this tank to a solenoid operated flow valve whose opposite side was connected to a 4 ft. length of 2 mm. polyethylene tubing. Variable pressures were provided and recorded by a mercury manometer in conjunction with two adjustable bleeder valves. Experimental control of conditioning stimuli relationships and individual stimuli were provided by stimulus selector switches and interval timers. The identity and duration of each stimulus were recorded by a polygraph event marker attached to a recording milliammeter. A manually operated switch activated the auditory stimulus relay during auditory threshold determinations.

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* It should be noted that both the present procedure and the apparatus used differ in many salient respects from those hitherto used and reported by the present author.

III. RESULTS AND DISCUSSION

Of the thirty-five sets of data, five had to be rejected for one of the following reasons: acquisition conditioning to the criterion not attained by sixty reinforced trials; failure of apparatus; failure to be available for testing 24 hr. later; failure to meet the criterion of pseudo-conditioning or initial sensitivity (two subjects).

The remaining thirty subjects were divided into two numerically equal groups according to the number of reinforcements required to attain the criterion of conditioning. These two groups were used for all further data analysis and comparisons. The two groups did not differ significantly with respect to age, hearing threshold, air-puff sensitivity threshold, spontaneous blink rate, or intelligence as measured by Raven's Progressive Matrices. No relationships were observed between air-puff sensitivities and any of the conditioning measures. The two groups differed significantly with respect to number of reinforcements required to attain the criterion of conditioning ($t = 3{\cdot}52, P < 0{\cdot}0$).

The extinction, spontaneous recovery and re-extinction patterns of these two groups are shown in Fig. 1. Because of the initial criterion of conditioning, common to all subjects, the extinction curves of both good and bad conditioning groups have, by definition, the same point of origin. The total number of extinction conditioned responses given by the good conditioners (taking the third test trial as the starting-point) is significantly greater than that given by the poor conditioners ($t = 2{\cdot}84$, $P < 0{\cdot}01$). Since the two sets of data start from the same point, and since the two curves have no grossly atypical points, it seems reasonable to conclude from this significant difference that they do not end at the same point. It follows, therefore, that the *slopes* of the two curves must also differ significantly as implied in the original prediction.

An essentially similar argument* can be applied to the recovery data by analysing the trials in two blocks. The first block consists of trials 1–5 inclusive, trial 5 being somewhat arbitrarily, but not unreasonably, taken as the middle of the probable cross-over range of the two curves; the second block consists of trials 6–16, trial 16 being the first point of final re-extinction for all subjects. The total number of re-emerged conditioned responses given by the poor conditioners in the first block is significantly *greater* than that given by the good conditioners ($t = 2{\cdot}19$, $P < 0{\cdot}02$).

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* The writer is indebted to David R. Saunders for his logical but ingeniously simple solutions to the problems presented in the trend analysis of these curves.

The total number of re-emerged conditioned responses given by the poor conditioners in the second block is significantly *less* than that given by the good conditioners ($t = 1{\cdot}98, P < 0{\cdot}05$). It seems reasonable, therefore, to conclude that the two recovery curves cross-over each other and differ in slope and initial and final phase relationships in the manner predicted.

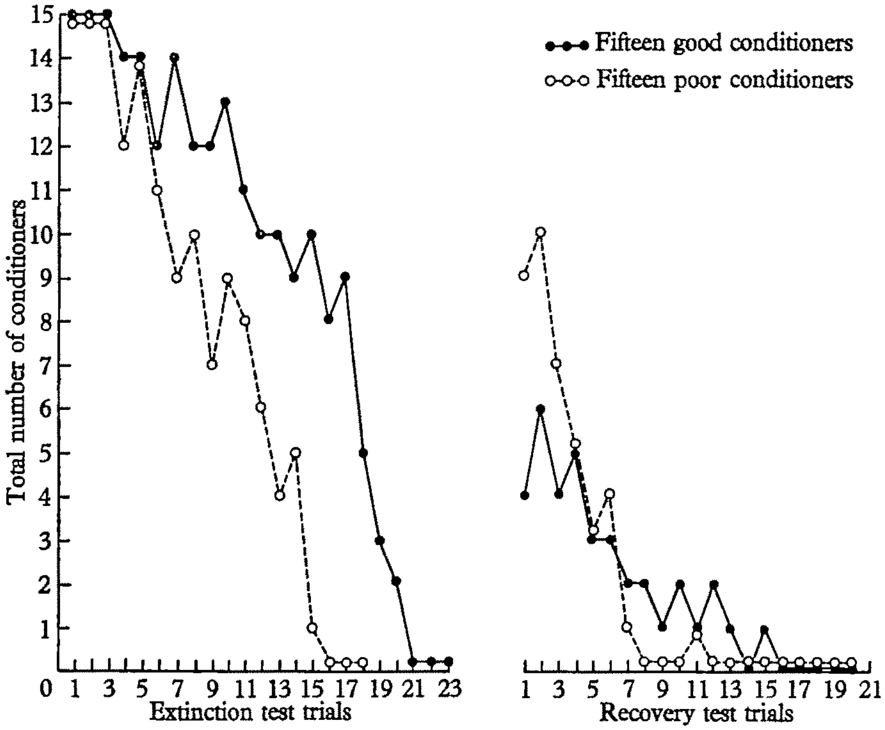

Fig. 1. Extinction of the conditioned eyeblink response to sound and its spontaneous recovery (re-emergence) after an interval of 24 hr.

A somewhat comparable study is that of Grant *et al.* (1958). Because of differences in procedure and intent it is not possible to make very meaningful comparisons; Grant *et al.* were interested in comparing the total number of re-emerged responses with the extinction responses, while the concern here is more with the reminiscence effect, i.e. with *initial* amounts of recovery, and then with subsequent differential rates of re-extinction. Using their index of the number of trials to re-extinction in spontaneous recovery as contrasted with the number of trials to initial extinction, the recovery rates for the good and bad conditioners after a test-retest interval of 24 hr. are 22 and 40 % respectively. The poor conditioners show relatively greater total recovery than the good conditioners. Perhaps the greater facility for re-extinction of the re-emerged conditioned responses of the poor conditioners is more than compensated for by the greater benefit obtained from the rest pauses.

Interpretation of the present findings would have been more unequivocal if more than two groups could have been employed in the analysis, variously equated with respect to the criteria of conditioning and extinction and with respect to the number of stimuli presented during these stages. In the present study the good and poor conditioners differ with respect to the number of extinction test trials actually received, a difference which may be pertinent to their conditioning behaviour 24 hr. later. It is also unknown whether the present effect holds for different conditioning parameters. For example, future studies will also need to explore the consequences of systematically varying the intensity of the unconditioned stimulus and of employing different inter-trial intervals. It must also be stressed that, in view of the surprisingly large amount of spontaneous recovery, and of the possibility that the nature of the activity during the rest pause can significantly modify the amount and course of the subsequent recovery and re-extinction (Liberman, 1944), any interpretation of the present findings solely in terms of some concept of reactive inhibition must be viewed with caution. Nevertheless, as far as the present data are concerned, any interpretation solely in terms of Spence's drive theory would appear to be even more unwarranted.

In conclusion, the clinical implications and possible applications of the present study may be noted. Perhaps, in behaviour therapy and psychotherapy based upon learning theory principles, it is those patients whose symptoms become readily extinguished who are most likely to produce a spontaneous re-emergence of the apparently extinguished symptoms. Fortunately, however, it appears that those who produce greatest re-emergence may also be those whose recrudesced symptoms most quickly re-extinguish. Within the context of clinical investigation and discussion of such possibilities the term 'spontaneous re-emergence' would seem to be more pertinent and less confusing, when applied to the reappearance of a symptom, than the experimental psychologist's customary but possibly clinically misleading term 'spontaneous recovery'.

It is a pleasure to acknowledge the contributions of a past colleague, Mr Leslie E. H. Lindahl, now at the University of London Institute of Psychiatry. Without his assistance in the development of the laboratory the experiment to be reported here would not have been accomplished so readily.

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A FURTHER ATTEMPT AT DELAYED GSR CONDITIONING

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A delayed conditioning schedule was employed so that latency and frequency changes of all GSRs occurring in the CS–UCS interval could be followed. Two experimental groups received initial (simultaneous) pairings of CS–UCS, and four control groups received initial CS series only, prior to the introduction of the delayed series.

There was no evidence of an increase in latency of GSRs over trials, and only a slight increase in frequency over the first few trials in the control groups; this latter effect was attributed to the disinhibiting or novel effect of changing from one stimulus series to another, and not to acquisition of a conditioned response. It was concluded that no gradual increase of CR strength over trials was demonstrated.

Highly significant correlations were obtained between measures of skin resistance responsiveness, such as spontaneous and orienting responses; this suggests that many so-called measures of GSR conditioning are probably related to some factor such as response sensitivity.

INTRODUCTION

The specific purpose of the present experiment is to determine whether the growth of a conditioned GSR can be followed during an acquisition procedure without giving test-trials of conditioned stimuli (CSs) alone. It follows a series of GSR experiments which have tried to clarify the criteria on which GSR conditioning is assessed, and which have been concerned with such problems as whether the GSR is conditioned according to the traditional definition of classical conditioning, and whether it shows a gradual increase in habit strength as a result of successive trials (Stern, Stewart & Winokur, 1961; Stewart, Stern, Winokur & Fredman, 1961).

A great deal of theorizing in conditioning has been carried out on the basis of relatively few conditioned response modalities (in man, almost entirely on the eyeblink reflex). It is of interest, therefore, to determine whether different types of responses—e.g. the GSR, which is an autonomic response produced largely by small changes in sweating—follows the same general laws that have been outlined for other response modalities. One particular problem which arises with the GSR is its rapid habituation even to quite intense UCSs, and its easy elicitation by any novel stimulus, such as a CS, however mild in intensity.

These problems can be by-passed to a certain extent by using only a short acquisition procedure with fairly long inter-trial intervals to minimize adaptation, and on the other hand by using long CS–UCS intervals so that orienting responses to the CS can be differentiated from longer-latency anticipatory responses occurring just prior to the UCS. By this means the development of the anticipatory CR can be followed during acquisition.

Although long CS–UCS intervals are frequently regarded as resulting in poor conditioning, they have been used quite successfully in GSR conditioning: Rodnick (1937) and Switzer (1934) used intervals of 20 and 16 sec., respectively, and claimed that the *latency* of the CR increased gradually with progressive trials: in the latter experiment, from an average of 5·09 sec. on the first four trials to an average of 8·21 sec. on trials 9–12. More recently, Stewart *et al.* (1961) reported an increase in the *frequency* of an anticipatory CR with successive trials. However, a subsequent experiment based on these findings and employing a 12 sec. CS–UCS interval failed to confirm either aspect of GSR conditioning: no evidence was obtained either of an increase in latency or in frequency of a conditioned GSR (Martin, 1963).

The question arises of whether the successful claims of delayed conditioning were, in fact, 'true' conditioning arising from the pairing of stimuli, or whether the schedules were such as to permit factors such as disinhibition to play a part in facilitating responsiveness. There were, however, certain methodological differences between the experiment failing to obtain delayed conditioning (Martin, 1963) and those experiments which were apparently successful. One major difference between it and the Rodnick and Switzer experiments was their use of a conditioning schedule which included initial *simultaneous pairings* of CS and UCS preceding the delayed series—a method Pavlov had used to obtain delayed conditioning. It seems reasonable to suppose that any failure to achieve delayed conditioning could be attributed to the absence of an initial series of CS–UCS pairings with a short, more optimal, interstimulus interval prior to the introduction of the delayed schedule, and the present experiment was planned to explore this factor.

METHOD

Subjects

Subjects were seventy-two male adolescent apprentices from a large industrial firm, age range 15 years 4 months to 20 years 4 months, mean age 17 years 9 months. They were allocated at random to one of six groups ($N = 12$ in each group), two groups being the experimental groups, and four control groups.

Apparatus

The apparatus used for measuring and recording skin resistance has been described previously (Martin, 1960*b*). It delivers a virtually constant current of 10 μA. Continuous recordings of skin-resistance levels and responses were obtained throughout the experimental session. Palm-to-palm recording was employed, electrodes being of silver-silver chloride, 24 mm. in diameter, used in conjunction with an electrode jelly made from agar-agar and a 0·5 % NaCl solution. The skin was prepared simply by wiping with acetone.

Procedure

In all, six groups of Ss were employed, two experimental and four control. The two experimental groups, (i) and (ii), received six and twelve initial paired short-interval CS–UCS trials, respectively; the control groups received initial CS trials only (either six or twelve). This raised the problem of what the duration of the CS should be in these groups.

If it were short, i.e. 2 sec., so that its duration was comparable with that of the CS during initial paired trials, then the sudden introduction of a 12 sec. CS–UCS interval in acquisition might well interfere with the S's perceptual set so that he would immediately give GSRs in the 12 sec. delayed CS–UCS acquisition procedure not because of conditioning, but because the novelty of a long stimulus could produce some kind of perceptual disparity which could facilitate responsiveness (Grings, 1960). On the other hand, if initial CS control duration were long, say of 12 sec., no direct comparison between experimental and control groups would be possible, since the function of the experimental groups was to receive initial training with short-interval CS–UCS pairing.

The only adequate course was to include control groups receiving long- and short-duration initial CS trials. The four control groups were therefore planned as follows: (iii) six initial CS trials of 2 sec. duration, (iv) twelve initial CS trials of 2 sec. duration, (v) six initial CS trials of 12 sec. duration, and (vi) twelve initial CS trials of 12 sec. duration.

Subjects were seated alone in a dark, sound-proof room, separated from *E* and the equipment. It was first explained to them that a recording would be obtained of their reactions to some stimuli—a light and a tone—and that they should remain still and relaxed. Electrodes were attached, headphones fitted, and assurances given that no electric shocks would be administered.

A 5 min. period was given to accommodate the S to the situation. Following this, the different initial treatments were started, either series of short-interval CS–UCS pairings or CSs alone. Then twelve delayed acquisition and twelve extinction trials were given at inter-trial intervals ranging from ½ to 2 min. Twelve trials were selected so that no marked adaptation would occur with so few trials; yet it seemed from previous experiments that this number should provide adequate opportunity both for an increase in CR latency or in frequency to occur.

The CS was a small, dim light from a 6 V. bulb placed in a large screen situated just in front of the subject. The UCS delivered to the S through headphones was a loud tone of approximately 1000 cyc./sec., 100 db. intensity, with a duration of ½ sec. Duration of the CS was 2 sec. in experimental groups' (i) and (ii) initial training trials, and either 2 or 12·5 sec. in the control groups' single CS trials. During delayed acquisition, duration of CS was 12·5 sec. for all groups; both CS and UCS terminated simultaneously—inter-stimulus intervals were, therefore, 1·5 sec. in the initial pairings for the experimental groups, and 12·0 sec. in the delayed pairings for all groups.

Measures obtained

The principal measures obtained were of mean level of skin resistance, number of spontaneous responses, and GSRs occurring to the CS during acquisition and extinction. To score as a response, deflections of $\frac{1}{16}$ in. or more were counted ($\frac{1}{16}$ in. was equivalent to a change of 1 kΩ).

RESULTS

The records were first examined for evidence of an increase in latency of GSRs occurring during the CS–UCS delayed acquisition period, but the results of this analysis were entirely negative. There was generally a clear orienting response to the CS, easily differentiated on the basis of its latency, which remained very consistent within Ss and between trials. Other GSRs occurring in the remainder of this period, however, showed great irregularity. Very responsive Ss (i.e. those whose records were characterized by many spontaneous responses) sometimes gave a continuous series of GSRs throughout the delayed CS–UCS period. Many Ss gave GSRs with short latencies (i.e. 5–6 sec.) as well as long latencies (10–12 sec.) even at the beginning of acquisition trials. There was no evidence of a gradual increase in latencies over successive trials as far as individual Ss were concerned.

In order, therefore, to systematize the GSRs given, they were classified into different latency groups (variables (vii), (viii), and (ix), see below). In all, the records were scored for thirteen variables, as follows:

(i) mean skin resistance during rest;

(ii) number of spontaneous responses during rest;

(iii) skin resistance level during 'treatment' (i.e. during initial series of paired CS–UCS or single CS trials);

(iv) spontaneous responses during 'treatment';

(v) skin resistance level during conditioning;

(vi) total number of spontaneous responses during conditioning;

(vii) number of orienting responses to the CS during acquisition (i.e. latencies 1·5–4·0 sec.);

(viii) number of responses with latencies 4·5–7·5 sec. during the CS–UCS period;

(ix) number of delayed responses, i.e. latencies 8·0–12·5 sec.;

(x) mean skin resistance during extinction;
(xi) spontaneous responses during extinction;
(xii) orienting responses to CS during extinction;
(xiii) delayed responses to CS during extinction.

Analyses of variance were first carried out on variables (i) and (ii)—mean skin resistance and spontaneous responses during rest—to check that there were no differences between groups prior to the different initial treatments. Neither F ratio was significant. When this had been ascertained, analyses of variance between groups were carried out on all the other variables, i.e. from (iii) to (xiii).

Only two were significant: (iii) skin resistance level and (iv) spontaneous responses during initial treatments, indicating that the two experimental groups which had received the UCS in initial paired trials had significantly lower resistances and gave significantly more spontaneous responses, presumably resulting from the more intense stimulation they received. The remainder of the analyses of variance on variables (v) to (xiii) were all non-significant. Thus they indicate that there were no significant effects on the total number of delayed GSRs, and other GSRs, either during acquisition or extinction as a result of (*a*) giving experimental groups initial training in CS–UCS short interval pairings; (*b*) giving control groups different durations of CSs (2 or 12 sec.) in initial series; or (*c*) giving different numbers (6 or 12) of initial trials.

Table 1. *Table of short, medium and long latency GSRs occurring in CS–UCS interval*

Acquisition trials	Groups 1, 2 (experimental)			Groups 3, 4 (control, 2 sec. CS)			Groups 5, 6 (control, 12 sec. CS)		
	S	M	L	S	M	L	S	M	L
1	22	10	8	15	4	10	13	5	4
2	20	10	9	22	12	13	24	6	14
3	18	6	12	24	14	16	23	11	12
4	20	8	9	21	10	20	19	9	14
5	18	5	7	21	7	15	15	6	13
6	18	6	6	20	8	16	19	9	11
7	18	4	7	20	6	11	17	6	10
8	18	5	6	20	5	16	14	5	10
9	12	3	7	17	1	9	11	6	4
10	18	2	11	16	5	9	14	4	7
11	18	4	5	19	5	10	12	5	11
12	17	2	11	15	5	10	15	4	7

S, GSRs with latency 1·5–4·0 sec. (incl.); M, GSRs with latency 4·5–7·5 sec. (incl.); L, GSRs with latency 8·0–12·5 sec. (incl.).

Following these analyses, the data were examined for change in frequency of GSRs over trials. Data on experimental and control groups considered separately are given in Table 1, and on all groups combined in Fig. 1.

It can be seen from Fig. 1 that the long-latency GSRs show a tendency to increase in frequency, from twenty-two Ss responding on trial 1, to forty-three Ss on trial 4. This increase might conceivably be considered as a 'conditioning curve' after the fashion of Stewart *et al.* (1961). There are several features in the data, however, which call for caution in this interpretation. First, is a tendency for medium-latency GSRs to increase over the first few trials—and the precise significance of these responses is uncertain, since they are not especially close in time either to stimulus onset or its termination; secondly, is the fact that both medium and long-latency increases are particularly marked in the control groups, and not, as predicted, in the experimental (see Table 1).

Relationships between variables

The inter-correlations between measures of skin resistance are given for all groups combined in Table 2. (Variables (iii) and (iv) are omitted since they differed significantly between groups as a result of the different initial treatments.) They show highly significant correlations between *levels* of skin resistance, i.e. in rest, acquisition and extinction, and between the different kinds of *responses*, i.e. orienting, spontaneous, delayed, etc. In general, skin resistance levels are not significantly related to number of responses given.

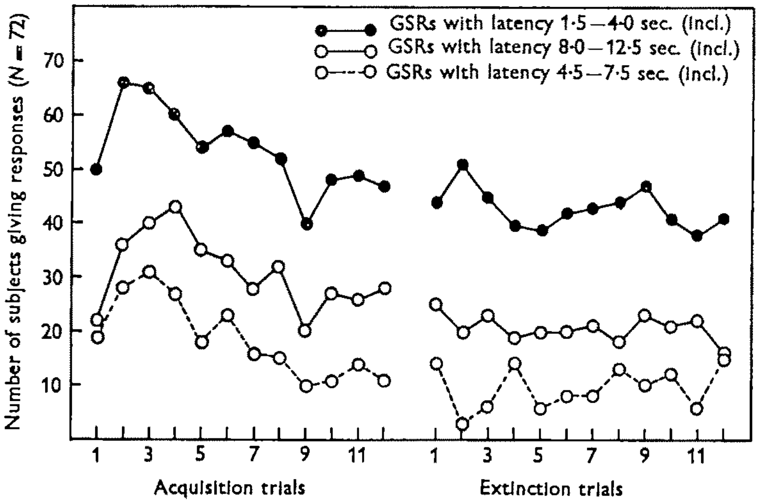

Fig. 1. GSRs occurring in CS–UCS interval during acquisition with delayed conditioning schedule, and (comparably) during extinction.

After the completion of this correlational analysis, the results of an experiment by Stern, Winokur, Stewart & Leonard (1963) were received which suggested a further way in which the present data might be considered. In their EDR conditioning experiment, two groups of Ss received different treatments, one a straightforward adaptation schedule followed by acquisition, and the second an identical schedule but with the interpolation of a UCS (shock) series between adaptation and acquisition schedules. They hypothesized that the series of shocks given to the experimental group prior to conditioning would have an activating effect, and should result in higher and more significant inter-correlations between the various kinds of GSRs (orienting, spontaneous, etc.).

The present experiment, though planned on a different basis, also included groups who had received different initial treatments, i.e. a CS-alone or CS–UCS series, and, as has been shown, the effect of the UCS was 'activating', or, to be specific, it produced a significantly lower skin resistance level and more spontaneous responses in

Table 2. *Inter-correlations between measures of skin resistance (all groups combined; $N = 72$)*

Variables	1	2	5	6	7	8	9	10	11	12	13
1. Mean skin resistance during initial rest	—	−0·1325	0·8632**	0·0483	0·1246	0·2464*	0·0450	0·7311**	0·1493	0·0038	0·0512
2. Spontaneous fluctuations during initial rest	—	—	−0·1217	0·5019**	0·3933**	0·3448**	0·4080**	−0·1715	0·4914**	0·3965**	0·3635**
5. Mean skin resistance during conditioning	—	—	—	−0·0321	−0·0152	0·1763	−0·0735	0·9241**	0·1783	−0·0119	0·0463
6. Spontaneous fluctuations during conditioning	—	—	—	—	0·4255**	0·5654**	0·5629**	−0·1220	0·6359**	0·4573**	0·4349**
7. Orienting responses to CS during conditioning	—	—	—	—	—	0·4694**	0·5146**	−0·0987	0·4823**	0·6507**	0·4138**
8. Medium latency responses during CS–UCS interval	—	—	—	—	—	—	0·5727**	0·7466**	0·4617**	0·5198**	0·4948**
9. Long latency responses during CS–UCS interval	—	—	—	—	—	—	—	−0·1497	0·4100**	0·4145**	0·5606**
10. Mean skin resistance during extinction	—	—	—	—	—	—	—	—	0·0851	−0·0851	0·0116
11. Spontaneous fluctuations during extinction	—	—	—	—	—	—	—	—	—	0·6492**	0·6178**
12. Orienting responses to CS during extinction	—	—	—	—	—	—	—	—	—	—	0·7077**
13. Delayed responses to CS during extinction	—	—	—	—	—	—	—	—	—	—	—

* significant at the 5% level; ** significant at the 1% level.

Table 3. *Inter-correlations between measures of skin resistance for (a) experimental groups* 1 *and* 2 (*N*: 24) *and (b) control groups* 3 *and* 4 (*N*: 24)

Variables		Groups 6	7	8	9	11	12	13
6. Spontaneous fluctuations during conditioning	(*a*)	—	0·5835**	0·5770**	0·6276**	0·5745**	0·5157**	0·4519**
	(*b*)	—	0·2732	0·6150**	0·4087*	0·7100**	0·5134**	0·5221**
7. Orienting responses to CS during conditioning	(*a*)	—	—	0·6560**	0·5318**	0·4521*	0·7578**	0·6556**
	(*b*)	—	—	0·3993	0·2542 .	0·4657*	0·5403**	0·3636
8. Medium latency responses during CS–UCS interval	(*a*)	—	—	—	0·6526	0·3597	0·5491**	0·5259**
	(*b*)	—	—	—	0·6316**	0·5364**	0·6078**	0·6301**
9. Long latency responses during CS–UCS interval	(*a*)	—	—	—	—	0·2447	0·4067*	0·5789**
	(*b*)	—	—	—	—	0·3030	0·3083	0·5923**
11. Spontaneous fluctuations during extinction	(*a*)	—	—	—	—	—	0·6655**	0·6842**
	(*b*)	—	—	—	—	—	0·6647**	0·5061**
12. Orienting responses to CS during extinction	(*a*)	—	—	—	—	—	—	0·6835**
	(*b*)	—	—	—	—	—	—	0·6777**
13. Delayed responses to CS during extinction	—	—	—	—	—	—	—	—

* significant at the 5 % level; ** significant at the 1 % level.

these groups. Both experimental groups were combined to form group (*a*), and correlations between variables were calculated on this group, and on the 2 sec. control groups (group (*b*)) separately. The results are given in Table 3.

There is a slight tendency towards more significant correlations in the 'activated' group, but these do not mainly include measures of spontaneous responses, as Stern *et al.* (1963) found.

DISCUSSION

The data provide little evidence for delayed conditioning, inasmuch as GSRs in the delayed CS–UCS interval show no increase in latency, and the increase in response frequency of long-latency GSRs over acquisition trials is slight. In any case, this increase occurs mainly in Ss who had received a series of CS-alone trials prior to acquisition trials, and not in those who received initial CS-UCS pairings; also, it has reached a maximum by the fourth trial and declines thereafter, an effect which could be due to disinhibition as a result of introducing a novel, intense stimulus (the UCS).

These data are therefore difficult to reconcile with the earlier studies on delayed conditioning; in view of the success of these other studies it can hardly be argued that long CS–UCS intervals are too far from optimal to achieve conditioning—in any case the evidence on this aspect of GSR conditioning is far from clear (Jones, 1962). There seems to be only one possible point of difference between the present experiment and those of Rodnick and Switzer, namely, that finger withdrawal followed their shock UCS. This is not specifically stated in their description of the experimental procedure, but incidental reference is made to 'finger retraction' which might have been occurring to the UCS. This suggestion has some support from Russian studies which show that autonomic responsiveness is very much strengthened when the Ss have to respond actively (for example, by pressing a button) to the conditioning stimuli (Razran, 1961).

On the other hand, the Rodnick and Switzer experiments do not pay attention to certain factors which were considered in the present experiment. They did not include control groups so that the effectiveness of their initial series of simultaneous pairings of CS and UCS cannot be assessed; neither did they report on spontaneous responses, or other GSRs occurring in the delayed CS–UCS interval. Finally, Rodnick recorded the Tarchanoff phenomenon of the GSR rather than the resistance change used here, and the instability of the Tarchanoff GSR as well as its biphasic nature are well recognized and could perhaps have given rise to difficulties in interpretation.

The problem with the present results is whether 'conditioning' has occurred at all, or whether we are dealing with some kind of pseudo-conditioning. Quite clearly there is *some* facilitative effect on responsiveness as a result of the UCS occurring in acquisition trials, for adaptation to the CS during acquisition trials is much more gradual than to initial CS-alone trials. But the general pattern of these 'acquisition' curves is not that which would be expected on the basis of increasing habit strength.

A number of hypotheses have been put forward to account for the pattern of GSR responsiveness observed during conditioning schedules, and two will be considered here. The first is that of Stewart *et al.* (1961) and Stern *et al.* (1963). These authors suggest that the decrements in response which they obtain are probably due to adaptation to the UCS arising from 'inhibition of reinforcement', and that this could possibly be avoided by spaced trials.

It might be that longer intervals would delay the adaptation process, although long inter-trial intervals are common in GSR conditioning schedules. It seems more likely, however, that the observed reduction in responsiveness is a function of decrement in drive strength. Spence (1958) holds that the strength of the UCS affects drive via the elicitation of an emotional response; the GSR is a component of this emotional—or, to be less specific, alerting—response, and all the available evidence on alerting responses shows that a *reduction regularly accompanies an unchanging stimulus series*. It might therefore be inferred that under certain conditions of autonomic response decrement there is a reduction in the hypothetical emotional response (one must be careful, of course, not to infer too much from a single response element). If this is so, the assessment of growth of habit strength (in the Hullian framework) would be no straightforward problem.

Many experimenters seek to modify this adaptation process by increasing UCS intensity during conditioning schedules, thereby manipulating the drive factor. Such manipulations introduce problems in interpreting the resulting data because they introduce an element of novelty into the situation and act as 'disinhibitors', which, as the Russians have well demonstrated in many classical conditioning situations, can produce response increment or arrest response decrement.

The second hypothesis, which is that of Grings (1960), suggests that perceptual set factors are *stabilized* by repeated CS–UCS trials; these presumably result in *decreased* autonomic responsiveness, since it is the *novel* or *disparate* stimulus which facilitates the alerting response of which the GSR is part.

There is no adequate evidence that 'awareness of disparity' is specifically a cognitive factor, and it equally follows either Grings's perceptual disparity view or the physiological evidence on alerting responses that an increase in delayed GSRs or any GSR is more likely to occur when the structure of the experimental situation suddenly changes, for example, when a single CS series changes into a paired CS–UCS series, when an acquisition schedule gives way to an extinction schedule, etc. It seems probable that a great deal of the evidence on effects of partial versus total reinforcement, and on interpolation of UCS ('pseudo-conditioning') trials during acquisition can be partially understood in terms of the *change* of stimulus pattern facilitating Ss' responsiveness.

It is impossible to turn to the GSR literature to evaluate this point of view because acquisition schedules are often excessively complicated, involving not only paired trials but 'test' trials of CSs alone, occasional UCS-alone trials, etc.; each change could be called 'disinhibiting' and prevent the S from developing any 'expectation' towards the stimulus schedule.

Unless schedules are kept very simple, and adequate controls incorporated, changing levels of alertness might well contribute a great deal to observed response increment and decrement. In addition, the specific nature of the response system being measured must be taken into account. The evidence, at least with GSRs, is now abundantly clear that some Ss are generally responsive and others unresponsive so that their performance on a conditioning schedule can be quite closely predicted from the number of spontaneous responses given during an initial rest period (see

Table 2; also Martin, 1960*a*, Stern *et al.* 1961). Thus ease of 'conditioning' (or responsiveness in a conditioning situation) seems to relate to response sensitivity. It need hardly be emphasized that the response measured represents the summated effect of a number of different CNS influences, including, perhaps, quite peripheral effects.

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PUBLICATIONS RECENTLY RECEIVED

The Perception of Causality. By A. Michotte, with a Foreword by R. C. Oldfield. Translated from *La Perception de la Causalité.* By T. R. and Elaine Miles. With a commentary by T. R. Miles. London: Methuen. Methuen's Manuals of Modern Psychology. 1963. Pp. xxiii + 425. 45*s.*

To employ Hirst's useful expression, this book might have been entitled, in translation, 'The Perceptual Consciousness of Causality', in order to avoid the impression that it is, at least primarily, about the perception of causality itself. Whether it is really about the perception of causality at all may be open to doubt. Michotte himself seems to be in two minds about the matter. He disclaims any intention to deal with philosophical questions about causality, and says that Hume, had he been able to take part in the experiments, would not have changed his philosophical views about causality at all, but only his view that the 'impression' of causality was inferential rather than immediate. Elsewhere, however, Michotte seems to be going beyond the purely phenomenological status of his psychological experiments, and says that a philosophically minded colleague pointed out that, in the experiments, 'You start off with an illusion, and use it to prove that causal impressions are real and objective' (p. 228). It is not easy to maintain the purely phenomenological outlook without ever allowing oneself to consider what may be beyond perceptual consciousness.

It is an open question, however, whether Michotte's experiments can be said to present illusions of causality. Indeed, in the reviewer's opinion they are no more illusions of causality when it is not really present than size constancy experiments give illusions of size which does not exist objectively. We see the object more nearly its 'real' size than the physicist would expect. In the Müller–Lyer illusion, on the contrary, owing to the very structure of the stimulus pattern, there is a true illusion, namely perceptual consciousness of a size difference which does not exist in objectively measurable reality. In Michotte's experiments (as in all experiments), causality exists behind the scenes, and perceptual consciousness is quite right in signalling its presence. It is rather in those cases in which the subject does not have the perceptual consciousness of causality, that there is an illusion that real causality is not present. But, of course, the mode of causality signalled by the perceptual consciousness is different from the real causality at work in the machines which cause the objects to move. Michotte says that we see the knife cutting the bread. As far as perceptual consciousness takes us, this is true. But what we 'really' mean by 'knife', 'bread' and 'cutting' would be difficult to define, and there would not be much left of our impression of cutting when such problems were settled. Therefore, one might well ask in how many cases of causality the causality we see is the causality really at work. In none, probably. And in trying to find the real causality which we think is what we see, we shall come back to Hume, and be found to agree that it may very well exist, but that we can never be sure either that it does or does not, while, if it does, it is probably never what we think we see.

Where Michotte scores his real success is in showing, by delightfully simple and elegant experiments, that the perceptual consciousness of causality occurs under certain definable and experimentally controllable conditions, although not always, and not under all conditions, and that when it does occur it is neither the product of the association of otherwise unrelated ideas or impressions nor of an inferential character. All the same, the idea of causality often is inferential, but Michotte is almost certainly right in claiming that the origin of the idea of causality in the individual's development arises from direct perceptual consciousness in certain situations, notably what he has called 'entraining' and 'launching', and is transferred to other situations in which it would not occur in immediate consciousness, but is then imported as an inference.

It is clear that Michotte's work is one of the great milestones in twentieth-century psychology, and we are grateful to the translators for putting into our hands this sensitive and careful translation, together with the useful commentaries, and the interesting Foreword by R. C. Oldfield.

R. W. PICKFORD

Le Développement Perceptif. By ROBERT FRANCÈS. Bibliothèque Scientifique Internationale. Paris: Presses Universitaires de France. 1962. Pp. iv+279. N.F. 16.

Psychologists who are interested mainly in modern developments in the study of perception as related to psychophysical processes and to information theory may consider the outlook of this book to be rather old-fashioned. Nevertheless, there is still some value in emphasizing that our perceptions of the world, although they are initially set in train by sensory information, are only partially determined by this. Indeed, recent work on the activities of the reticular formation suggests that our awareness of and our reactions to this information, though to some extent a function of repetition and habituation, do depend also on other factors such as interest and affect.

Of particular value in the author's thesis is his distinction between identification and discrimination. In the former, the subject is less concerned with the exact perception of details than with the integration together of all the relevant sensory aspects, which may be corroborative even if redundant, into patterns which are assimilated to conceptual classes, the latter having been developed and refined through action and use. Moreover, the identifications depend also on contextual effects: perception of the immediate situation as a whole, and attitude to it; and expectations based on knowledge, previous experience and training, response tendencies and feedback. By contrast, in discrimination the subject is concerned to analyse the total experience carefully and accurately and to extract from it the particular details relevant to his task. Furthermore, in the development of discrimination contextual effects may be progressively rejected as impairing its objectivity and accuracy. In everyday life, ease and rapidity of identification are of major importance in adaptation to the environment, and extreme accuracy of discrimination is required only in certain specialized tasks and situations. Rather unfortunately, however, there has been a tendency on the part of many psychologists to direct their attention exclusively to the study of accurate discrimination of detail; and to resent the intrusion of contextual effects, labelling them as 'noise'. Even when there has been no deliberate intention of this kind, the experimental constraints have sometimes forced the subject to discriminate details, and then to make conscious inferences from them such as would seldom be made in more normal circumstances. Again, what is perceived is in fact much affected by contextual factors which seldom operate in everyday life: 'set' produced by instructions, procedure and general attitude to the experimental situation.

In his discussion of the extensive literature on perception, the author does not maintain the distinction between identification and discrimination very consistently, perhaps because of the paucity of experimental investigation of identification; but he might have devoted more consideration to the effects of experimental 'set'. In adult perception, it is often difficult to distinguish in practice between the two processes; the more the pity that there is little discussion in the book of children's perceptions, in which they are more distinct. However, there is an extensive and useful exposition of the effects of contextual factors, in psychophysical experiments, on simple perception of number, shape and movement, and on the constancies. The effects of cognitive contextual factors are rather less fully explored than those of social and other motivational and affective influences. However, the author does raise the problem, suggested by Michotte's work, of the conflict which may arise between immediate perception and knowledge about the circumstances.

With regard to the effects of motivation, the author maintains that these in some cases operate merely to facilitate or inhibit perception, but in others may in addition shape or modify the percept itself, as in the perceptions of hungry subjects. But he is perhaps a little too ready to believe that percepts themselves are modified, rather than judgements or responses. Neither does he always recognize that non-veridical percepts may result from the impoverished conditions of experiment; and that in normal circumstances they would be discarded in their initial stages of development because they were incongruent with the total situation and the context of knowledge and past experience.

However, the author must be congratulated for formulating the distinction between identification and discrimination, and for his very thorough discussion of so many contextual factors which operate in perception. His argument is not always easy to follow, but is well worth careful consideration. It is a pity that there is no subject index.

M. D. VERNON

The Logic of Preference. By G. H. VON WRIGHT. Edinburgh: University Press, 1963. Pp. 68. 10*s.* 6*d.*

In this essay, which is an expanded version of one of a series of four lectures on *Ethics and Logic* given in May 1962 in the University of Edinburgh, Von Wright studies from a logical point of view the notion of liking one thing better than another. This notion he considers to be of some importance, since it forms a common root of aesthetic, moral and economic valuations. He disagrees with some contemporary philosophers who maintain that norms and values are by their nature a-logical, and not amenable to the methods of formal logic. We can, he argues, make a logical study of norms. This approach, he contends, has been neglected in the past.

The formal theory he constructs deals with preferences between states of affairs, and rests on five formal principles. He presents it in the form of a technique for deciding whether a preference statement does or does not express a proposition which is logically true. The preferences studied are a subject's intrinsic preferences on one occasion only. The subject's reasons for holding these preferences and the possibility of changes in them are expressly excluded from his study. Though Von Wright recognizes that some preferences are intransitive, that is to say that, though we may prefer A to B and B to C, we may yet nevertheless prefer C to A, he rules out this possibility. The subject's preferences are hence assumed to be made on a purely rational basis.

A basic assumption of Von Wright's system is that there are elementary states of affairs, and his logic of preference is a logical theory of unconditioned intrinsic preferences between such states of affairs. He counters the criticism that this assumption involves a logical atomism by saying that for the purpose of a formal calculus the difficulty can be evaded by stipulating that the unconditionality of preferences shall be taken as relative to a given set of descriptions of generic states of affairs. However, this still would not get over the difficulty that these states of affairs remain functionally unrelated to each other.

For Von Wright moral philosophy is a purely conceptual study: it is concerned with normative and value concepts. He is fully aware, however, that such concepts as good or evil, right or duty, must be based on a more thorough examination of the notions of need, want, decision and choice. Von Wright's account is, however, non-empirical, since he would relate value concepts only to the abstract concepts of need, want, etc., and not to the phenomena themselves from which they obtain at least part of their meaning. It is—to quote a fashionable phrase—a second-order study. But since the meanings we attach to such concepts change with our increasing understanding of human nature, any calculus based on them would have to be continuously revised with the advance of knowledge.

The essay itself is written in a limpid logical style. The various implications of preference statements are studied and clearly worked out. The abstract model is impeccable, but what we, however, want to know is how far it applies to the facts. Unless it does so adequately, the whole study becomes a purely logical exercise. What makes this model difficult to accept is its central assumption, namely, that there are such things as simple preferences which relate to atomic states of affairs. This seems to be at variance with actual fact. It is doubtful whether the notion of a pure preference is as simple as Von Wright makes it out to be. As preferences vary from context to context, the notion of a pure preference seems to be a high order abstraction. W. MAYS

Concept Learning: an Information Processing Problem. By EARL B. HUNT. New York: John Wiley and Sons. Pp. x+286. 57*s.*

This monograph re-examines the problem of the psychological nature of concepts and the manner of their formation in the light of cybernetic ideas and computer techniques. It displays a sustained high level of thought and scholarship and has a wealth of material and many new viewpoints, not only on its main field but on surrounding areas. Particular mention should be made of the approach to association and schematic organization of cognitive material in chapter 7 and of the suggested detailed steps in identification set out in chapter 8. The book deserves to be studied by anyone who is concerned with questions of thinking, problem-solving, perceptual identification or memory.

Whether it will make the impact it should is, however, doubtful because it is almost incredibly tedious to read. The way in which it is written falls between the styles of the long theoretical article in which evidence is outlined but not considered in detail and the fully documented textbook, so that it is longer than the expert needs, but not explicit enough for those lacking a wide background knowledge. The elaborate terminology gives the impression of profound discussion at a distance almost always just beyond the reach of the reader. Nor does the layout help: there are seven chapters surveying various background areas and preliminary questions before the two chapters and final note in which the book comes into focus. The reader will find his task considerably easier if he starts at chapter 7 and reads to the end of the book before attempting the earlier parts.

One further point of both praise and criticism should be made. The book is an important and brave attempt to break away from some of the traditional ideas about perception and learning which have dominated psychology over the last quarter century or so. The direction in which it is moving has a good deal in common with what have become accepted ways of thinking in this country under the influence of Bartlett, Craik, Hick, and those who have followed them. In view of this it is surprising that the author has not paid more attention to British sources. In particular he makes no mention of Craik's *The Nature of Explanation*, which is directly relevant to his ideas and has been acknowledged as having profoundly influenced thought in this area on both sides of the Atlantic. The omission of such sources is a pity because in them, if I understand the author aright, he would have found that some of his problems had already been largely solved, and he would thus have been able both to sharpen and to lighten his own treatment of them.

A. T. WELFORD

Lehrbuch der experimentellen Psychologie. By RICHARD MEILI and HUBERT ROHRACHER. Bern and Stuttgart: Huber. 1963. Pp. xi+480. DM. 48.

This is the first proper text-book in the German language to emerge since the war and it will undoubtedly be widely adopted in the German-speaking countries. Each editor has contributed a chapter, Rohracher on memory and learning, Meili on thinking, including the factor analytic approach. Meili has also written an introductory chapter on experimentation in psychology. The other chapters have been contributed by various experts; outstanding amongst them are chapters by Ekman on psychophysics and psychological measurement, by I. Kohler on perception, and by Pawlik on statistical methods. Other chapters deal with emotion, motivation, personality and social psychology. The book as a whole thus deviates from its pre-war predecessors by adding to the field of experimental psychology, strictly so-called, the areas of social psychology and individual differences.

The contents of the book do not, therefore, differ very much from those of a typical American text-book, and on the whole the treatment of topics is very similar too. There is not the usual disregard of non-German sources; most of the well-known Anglo-American writers are adequately represented. The discussion is usually up to date, although there are some odd lapses; the chapter on experimental aesthetics, for instance, takes no account of the studies of the last thirty years. The chapter on emotion, too, seems a little archaic. There is very little on autonomic reactivity and such important concepts as Lacey's doctrine of 'response specificity' is not mentioned. Instead we have a picture of a very ancient kymograph and Marey tambour which must have been taken at least fifty years ago!

However, on the whole the quality of the book is high, and students approaching psychology for the first time are likely to receive a sound, non-doctrinaire introduction which will well qualify them for further reading and study. The editors have lived up to their high reputation for excellence of judgement and ability to distinguish the important from the ephemeral.

H. J. EYSENCK

Aspects of the Theory of Artificial Intelligence. Edited by C. A. MUSES. New York: Plenum Press. 1962. Pp. x+283. $10.00.

This volume consists of the Proceedings of the First International Symposium on Biosimulation, held in Locarno in the summer of 1960. The term 'biosimulation' was coined by the editor at the time he proposed the holding of this conference. The dust-jacket refers to 'biosimulation or advanced cybernetics' as an 'exciting new science'. It would be futile to attempt to define its boundaries. The scope of the symposium is, in fact, as wide as might be expected from the contributions of such eminent cyberneticians as W. R. Ashby, H. von Foerster, D. M. Mackay and W. S. McCulloch.

There are, in all, ten diverse contributions (called, without much justification, chapters) and some appendices. Some of the papers are of considerable interest to psychologists. One is *Learning in a Nondigital Environment* by A. M. Andrew. Now digital computers have, of course, been programmed to learn, but the learning of organisms depends on continuous, non-digital communication with the environment; Dr Andrew has presented a theoretical engineering system for continuous learning. Another interesting paper is *The Simulation of Learning and Decision-making Behavior* by Gordon Pask. Prof. Mackay in *Theoretical Models of Space Perception* has considered models of perception of change and perceptual stability in terms of the flow of information.

Other papers in this symposium are of less immediate interest to psychologists. Thus, self-reproducing systems, which should not be regarded as exclusively biological, are discussed by Dr Ashby. Dr R. L. Beurle looks into the 'possibility of explaining some of the adaptive behavior of living organisms on the basis of bulk properties of networks which may have a very large random factor in their construction'. The symposium includes a down-to-earth technological paper by K. R. Shoulders *On Microelectronic Components, Interconnections, and System Fabrication*; but the symposium contains also some very wide-ranging papers, which perhaps may best be called philosophical, for example, *The Logic of Biosimulation* by the editor of the volume, Dr Muses. In spite of its uneven character, this book should be read by anyone interested in keeping abreast of developments in cybernetics and related fields.

W. SLUCKIN

Birth to Maturity: A Study in Psychological Development. By JEROME KAGAN and HOWARD A. MOSS. New York and London: Wiley. 1963. Pp. xiii+381. 65*s*.

In so far as psychology is concerned with prediction of behavioural tendencies, this book should be of cardinal importance. It is a correlational study of the stability of behaviour of the same group of eighty-nine individuals measured at various periods of childhood, and seventy-one of them in early adult life. One investigator rated behaviour on a number of scales for each of four periods between birth and 14 years, basing the assessments on detailed narrative accounts of frequent observations of the child at home, camp and school and on annual interviews with child and with mother. The other author interviewed and rated the subjects as young adults without prior knowledge of their histories. Various tests of ability and personality and assessments of maternal practices in child rearing were added to the programme, and conclusions are drawn concerning the value of these diverse research tools as well as the inter-relationships of the variables they measure.

The output of this massive programme takes the form of several thousand correlations, which are presented, along with test protocols and the like, in thirty-three appendices. The authors' problem has been to impose on this mass of information some pattern that the reader might grasp without being overwhelmed with detail. This they have done by systematic presentation and by highlighting consistent results, without minimizing the equally numerous inconsistencies. They are not concerned to erect new theories on these results, which indeed scarcely lend themselves to such use, but do suggest interpretations based on existing theory of an unprovocative kind.

Some of the findings are none the less of considerable theoretical as well as practical interest. For instance, several variables assessed in the early school years, and a few even before school age, were found to be better predictors of adult behaviour than were corresponding assessments at later ages. Similar delayed ('sleeper') effects were observed in the outcome in adult life of mothers' early attitudes, especially toward their infant daughters. Marked sex differences were found both in patterns of development and in correlations with environmental factors; some of these make sense in terms of cultural conditioning.

But with changing forms of behaviour, arbitrary semantic labels, and large numbers of correlation coefficients with a wide margin of error, there is danger of placing too much weight on the correlations that happen to come highest. The authors do not ignore this pitfall; in some areas they have carried out limited replication studies. Their book is chiefly valuable as a workmanlike manual for reference and, one hopes, a stimulus to further investigations in this difficult but important field.

T. W. MOORE

Motivation as Related to Personality. By DOROTHY RETHLINGSHAFER. New York and London: McGraw-Hill. 1963. Pp. ix+388. 62*s.*

This book is an attempt to review recent research on motivation, chiefly in humans but including relevant animal experiments. The author has cast her net very widely indeed. There is a chapter entitled 'The Biological Approach to Motivation' which deals with such topics as the 'registration of tissue needs', 'homeostasis', and 'regulation'. Other chapter headings include 'The Investigative–Explorative Orientation', 'Activation' (a term used in both physiological and behavioural senses), 'Personal Pace' (individual differences in activation) 'Goals,' 'Tension', and 'Anxiety—A Specific Tension State'. Also discussed are drive, the selection of stimuli and responses, motivational determinants of cognitive activities, and motivation in learning and retention.

In view of the breadth of material covered, it is perhaps not surprising that the treatment should seem at times superficial. There is a lot of terminological confusion in some of these fields and the book does little to straighten it out. One feels that a good opportunity has been lost. For example, terms like *activation, level of activity, activity stream* and *arousal* occur frequently but nowhere are the various concepts involved clearly delineated. Again, in the chapter on the biological approach we read that 'homeostasis may be used quite broadly to refer to general optimal conditions like a pleasant sweet taste, freedom from anxiety, or having a high status'. This seems to be an unjustifiable broadening of a useful physiological concept. To be fair it should be added that there is a good, if brief, account of homeostasis in its proper physiological sense, adapted from another author. In all the chapters there are useful factual summaries of relevant experimental work. These form the most valuable part of the book and combined with the list of references could serve as an introduction to the literature on motivation. It is unfortunate that they were not put to better use in providing a more coherent account of the whole field.

J. G. INGHAM

This Island Now—The B.B.C. Reith Lectures, 1962. By G. M. CARSTAIRS. London: The Hogarth Press. 1963. Pp. 103. 10*s.* 6*d.*

Most British psychologists are no doubt acquainted with these lectures and with much of the publicity they have received in the press—from the great national dailies and weeklies to the parish magazines. Now that this excitement has given way to other excitements we can re-read these lectures in a relaxed frame of mind. They should certainly be re-read notwithstanding the fact that they contain little that is new and perhaps little with which most psychologists would strongly disagree. Their importance lay partly in the fact that they were given over the air and told the general public many things which hitherto social scientists had not been allowed to say over the air, or which, even if they had been allowed to, many would not have dared to say.

The themes of these wide ranging lectures concern, *inter alia*, changes in child rearing practices, in adolescent sexual behaviour, in the role of women in society and changes in the British character. The lectures conclude by discussing the relations between religion and psychiatry and the interplay of science and the arts. This last topic, science and the arts, is innocent enough and not calculated to arouse strong passions but most of the rest could be, and has proved to be, controversial.

The thesis that charity is more important than chastity is not, perhaps, controversial as a general thesis; but the more specific thesis—that pre-marital sexual relations may be positively conducive to a happy married life—is more challenging. It is challenging to social psychologists and others who may be interested in research relevant to the validation or disproof of this interesting hypothesis. Not less challenging to social psychologists and sociologists are Prof. Carstair's observations on the changing pattern of married life. It will be a great pity if the title of these lectures limits the circulation of this book. What is happening *in* this island now is happening elsewhere. What is happening *to* this island now is, among other things, that it is ceasing to be an island in any but a narrow geographical sense. This book should be read in China, in Russia, in the United States and Ghana, as well as in the old-fashioned countries such as Denmark, Holland, Norway and Sweden which are all contributing in various ways to the promotion of useful changes not only in this island but all over the world.

C. A. MACE

Scientific Creativity: Its Recognition and Development. Edited by C. W. TAYLOR and F. BARRON. New York and London: Wiley. 1963. Pp. xxiv+419. 60*s*.

This important book is a landmark in the study of creative scientific thinking. For one thing, it provides a useful conspectus of its many-sided topic and an extensive bibliography through which the reader can trace possibly all relevant literature. For another, it reports several pieces of research which successfully combine rigorous method with sensitivity to the complex subtlety of the phenomena being explored. Some of this original work is inconclusive but it can fairly be said that all of it helps to sharpen some problem area and to refine our procedures of investigation. Taken as a whole, the book amply confirms that empirical inquiries into issues of human creativity are feasible and are making definite, albeit slow, progress.

The book has its origins in three conferences supported by the U.S. National Science Foundation and held at Utah University in 1955, 1957 and 1959. Thirty contributions have been selected from the Proceedings of these conferences, edited, revised by their authors, and brought together in this volume. The papers are grouped, sometimes uneasily, into four parts. Part One deals, appropriately enough, with establishing criteria of creativity and discovering predictors of these criteria: considerations span phenomenology, psychometrics, and correlational analyses of the grounds on which various professional groups judge people to be creative. Part Two is devoted to intellectual, personality and motivational characteristics of scientists: the studies here are diverse in nature and yet lead to impressively close agreement over the distinctive traits of productive scientists. Part Three concerns environmental influences ranging in breadth from the cultural and historical, through the social and educational, to the specifically situational. Part Four is, wisely, the briefest of the four parts for it is devoted to 'Theoretical Analyses of Process'. A concluding chapter overviews the whole book, summarizes some of its main findings and delineates specific problems likely to repay study at the present time.

This volume does not fall into the familiar contemporary class of being an ephemeral collection of unevenly competent and dubiously related papers. It makes a distinct contribution to the challenging topic specified in its title, and will probably be a standard source for some years to come. It is a book which can be shown without embarrassment to any of the highly talented people whose functioning provides the subject-matter of scientific creativity. I. M. L. HUNTER

Der Konstitutionstypus. By KLAUS CONRAD. Berlin: Springer. 1963. Pp. viii+242. DM. 58.

Conrad's book is the best critical survey available of Kretschmer's system; although brief discussions are included of Sheldon's work and occasionally that of others, the stress is predominantly on the German contribution. Conrad follows Kretschmer by no means blindly; he has a sharp eye for weaknesses and contradictions, such as the master's fateful failure to distinguish the leptosomatic from the asthenic body-build and the resulting difficulty with the exact position of the athletic type. However, Conrad still accepts parts of Kretschmer's teaching which must be regarded as very doubtful in view of the experimental work reported by Payne and Brengelmann; as their contribution has appeared in German it seems strange that it should not even be mentioned by Conrad.

However good the book may be as a summary of Kretschmer's work its main importance lies undoubtedly in two original contributions made by the writer which take him well beyond Kretschmer. The first of these contributions lies in the recognition that apart from a 'Primärvariante' there is also a 'Sekundärvariante' in the description of body-build; as we would put it, in the language of factor analysis, there is a general factor of size as well as a bipolar factor of proportion. Conrad in fact quotes, in support of his thesis, '...the factor analytic studies of Rees and Eysenck...'—this unfortunately being the only mention of the empirical use of correlational and factorial analysis. Conrad could have strengthened his case considerably by discussing the very large literature in much greater detail.

The other contribution Conrad makes is of even greater importance. He puts it in a series of laws which are quite rigorously deduced from observational studies. The first law says: 'In its morphological proportions "pyknomorph" body-build is to "leptomorph" body-build as is an ontogenetically early to an ontogenetically later set of proportions.' He also proves the inverse of this: 'Proportions which do not change with age do not show differences between types.' He then goes on to show that the ontogenetic principle of body-build applies not only to morphology but also to physiological reactions, and lastly that it can also be applied to psychological reactions in a variety of fields. This theory is so important and so original that anyone interested in body-build and its relation to personality should not miss reading this important book; it is unfortunate that Conrad has not published this theory quite separately from the more humdrum review of the Kretschmerian system; as it is it will undoubtedly receive less attention than it undoubtedly deserves.

H. J. EYSENCK

Story Sequence Analysis. A New Method of Measuring Motivation and Predicting Achievement. By MAGDA B. ARNOLD. New York and London: Columbia University Press. 1962. Pp. vii+287. 56*s*.

In this book Dr Arnold concentrates on the use of the TAT for the single purpose of predicting achievement. The work is based on several hundred records taken from elementary, secondary and college students, teachers, business executives and naval ratings. Dr Arnold expounds her theory of motives, values and attitudes, and makes the point that 'motives include...a choice of action'. She bypasses drives, ego processes, problems of identification and the stimulus value of the pictures, and maintains that in the TAT 'motivating tendencies... shape the story action and are expressed in the story outcome'. Her method consists in abstracting an 'import' from plot and outcome of each story; as the same problem often appears in several consecutive stories, the imports should then be linked whenever possible; if all the imports are read in sequence the individual's intentions for action emerge. Dr Arnold found empirically that the imports of 'high achievers' were distinguishable from those of 'low achievers' (e.g. very efficient teachers and very inefficient teachers). She evolved scoring criteria for positive and negative motivation which enable her to predict also the achievement of people whose motivation is neither strongly positive nor negative. She gives the imports and scores of some 150 individual stories but this includes the full records of only 8 people. More complete records for scoring practice would have been useful. Five validation studies are reported and discussed but further research on test–retest reliability seems desirable.

To abstract the correct import may present difficulties even to the experienced psychologist. Provided the method can be mastered without tuition, it may well prove valuable for vocational selection and rehabilitation.

A. KALDEGG

The Rorschach Technique: an Introductory Manual. By BRUNO KLOPFER and HELEN H. DAVIDSON. New York: Harcourt, Brace and World. 1962. Pp. 245. $8.50.

Any publication by Bruno Klopfer must be welcome to those practising the Rorschach technique. The present volume, written with the able collaboration of Helen Davidson, is designed to replace the work of Klopfer and Kelley published under the same title in 1942, adding to this a number of the refinements of assessment described at greater length in the 'Developments' publications of 1954 and 1956.

This up-to-date introductory manual provides a pleasant contrast to that of 1942 in its greatly increased clarity and precision of phrase, which make for easy reading and assimilation. After a short historical survey, and notes upon the general theory and method of personality appraisal, the method of test administration and scoring are described in detail, together with a chapter on the use of the record sheet provided. The clinical sensitivity of Klopfer, and perhaps the skill which he has acquired as a Jungian analyst, seem reflected better than in any of his previous text-books when he discusses interpersonal relationships briefly under the heading 'The Examination Atmosphere'. Indeed, many of his admirers, including the present writer, could wish that he had allowed himself more space to expand upon this subject.

Many examples of scoring are given, and will be useful to students. Case studies, while fully set out, are in accordance with an introductory approach wisely limited to comparatively simple examples which illustrate theories of assessment described in an earlier chapter. Those whose work is concerned with children will be glad that the study of a boy of 10 has been included as an appendix.

In the section on scoring, the advice to give all additional determinants a common rating of 0·5 may not be universally acceptable, since these additionals vary so greatly in their perceptual significance. In a response such as leaping flames, directed to a red area, the determining factors of movement and colour are usually inseparable and apparently of equal importance to the perceiver, in contrast to other percepts which are scored as additions either because they refer only to a small part of the area used, or because they are not seen until the second showing. However, the question as to whether additionals should be weighted is another story. Disagreement on this small point should in no way detract from appreciation of a contribution to Rorschach literature upon which its authors are indeed to be warmly congratulated.

THEODORA ALCOCK

Decisions, Values and Groups. Vol. 2, Proceedings of a Conference sponsored by the Air Force Office of Scientific Research held at the University of Mexico. Edited by NORMAN F. WASHBURNE. Oxford: Pergamon. 1962. Pp. 521. £5.

A review of the first volume, which presented the proceedings of an earlier conference, has already appeared in this *Journal* (1961, **52** (2), 191–2). Unlike the first volume, which was subdivided into five sections each dealing with a specific group of topics, the second volume displays no such pattern. There are twenty-two chapters, beginning with a general discussion on a topic described as 'The Inter-disciplinary Approach and The Concept of Behavioural Science', and concluding with a chapter entitled 'Internal versus External Control of Reinforcements'. In the latter, the three authors state that they include in this expression the notion of chance versus skill, but they nowhere refer to a variety of experiments carried out on this theme in this country. This regrettable tendency to omit any reference to work outside the U.S. characterizes the book as a whole, as it did the first volume, a provincialism on the part of so many American investigators which seems very hard to eradicate.

The remaining twenty chapters deal with such diverse subjects as: a programme for research in social psychology; suggestions for quantitative methods; an examination of the concept of intelligence; cognitive structure and consistency; 'natural' sociology; academic prestige; opinion leadership; group performance; self-esteem and aggression; deterministic theories; communication; and decision-making and conflict.

Clearly, the net is cast wide. Some chapters, for example, that by H. W. Reicken on experiments in social psychology, amount to detailed schemes for research projects such as might be submitted to grant-distributing organizations. Others propose highly specific solutions to specific problems such as that by P. D. Dubois and D. Gold which relates to the partitioning of chi square and the use of factor analysis. Some are in the nature of a critique, and a few describe experiments and their outcome. The two chapters by E. Rose possibly have the widest appeal. They describe the results of an analysis of words undertaken to trace the derivation of 'folk' sociological concepts.

Mr C. E. Hutchinson, who writes the Foreword, is at pains to justify the need for publishing the volume in view of the fact that, as he tells us, the results will come to light in at least three other forms, in particular as research proposals to many public and private agencies, and 'in a large number of research reports, monographs and articles which are now coming out and will continue to appear as journal space becomes available'. Why then, we may still ask, is it necessary to duplicate all this effort for one and the same audience? The question is all the more pertinent since the book, whatever the intentions of its planners, lacks any semblance of unity. I suspect that many readers will not be reassured by Mr Hutchinson's remarks. The book will still seem to them like an assembly of articles from a variety of journals without any clear or selective principle. To have to pay £5 for a compilation of this sort seems to be overtaxing the ingenuousness both of social scientists and of the librarians who provide for their bibliographical needs.

JOHN COHEN

Psychology and Religion. By G. STEPHEN SPINKS. London: Methuen. 1963. Pp. xv+221. 28*s*.

The recent *succès fou* of the Bishop of Woolwich's book *Honest to God* has reflected the present interest in the basis of religious belief, so that any book on the psychology of religion is likely to be read by a wider public than the professional psychologist and theologian. In this respect

Dr Spinks's book will not disappoint, as it is not unduly technical from either a psychological or a theological point of view.

The tone is set by two sentences in the preface: 'Religion begins in experience. It is this fact that makes it a proper subject for psychological study'. It is thus assumed that religion involves individual experiences which, as is made clear later in the book, are more than experiences of group activity, or the authoritative statements of priest or Bible, and involve 'the individual's apprehension of some supreme Object, Power or Principle'. To consider that experiences of this sort are a proper subject for psychological study also makes it clear that Dr Spinks's approach is no behaviouristic one: indeed he firmly dismisses behaviourism in a footnote as being 'wholly unsatisfactory'.

This is an admirable start to the book; firm and unambiguous. The first part on 'Psychological Theories and Religion' is an erudite review and discussion on the history and origins of religion, and its relation to various psychological concepts. The second part deals with 'Psychology and Religious practices', and discusses the growth of religious consciousness, and prayer, worship and religious experience.

There seems, however, to be one important omission in the references to relevant psychological research. There is no mention of the important work of A. H. Maslow (*Toward a Psychology of Being*, 1962. New York: Van Nostrand) in the fields of perception and personality. Maslow's description of D-cognition and B-cognition shows how modern theories of perception can throw light on the psychological factors in prayer and worship. His studies of well-integrated people are also directly relevant to the field.

It is research of this kind which is so badly needed. Dr Spinks permits himself some fascinating speculations about the religious orientation of the three Sheldonian temperamental types. 'The viscerotonic type', he says, 'inclines towards sacramentalism', 'The somatotonic type shows a strong inclination to make converts', 'The history of mysticism is mainly the record of the cerebrotonic temperament'. But what is the evidence for this? Do our High Anglicans tend to be fat, jolly priests? Do our evangelicals tend to be muscular Christians? Do our Quakers tend to be brooding intellectuals? It would be nice to know. Is it too much to hope that the next crop of books on the psychology of religion will be devoted to experimental studies of the theories and speculations which have been advanced by books such as this? RALPH HETHERINGTON

Mathematical Models in Small Group Processes. Edited by JOAN H. CRISWELL, HERBERT SOLOMON and PATRICK SUPPES. Stanford: University Press, 1962. Pp. x+361. 78*s*.

This book contains 22 articles on widely different aspects of model making in small group processes.

If, in experimental situations, the stimuli leading to the responses are arranged along a scale ranging from strong control by the experimenter at one end to freedom of choice and the use of skill by the subject at the other, the papers included in this book vary from those of the learning theorists at one extreme to Moore and Anderson's autoelic models and Back's communication processes at the other. Starting at the permissive end of the scale, Carterette and Wyman employ various psycho-physical and learning approaches to investigate perceptual factors in conformity experiments. Although they find that the stimulus-sampling concept is applicable to psychological processes the findings appear to raise difficult problems for both signal detection and statistical learning theory. Suppes and Schlag-Rey, who analyse social conformity in terms of generalized conditioning models, also test social psychological theory by means of perceptual judgement. However, in Rosenberg's experiments the subject is allowed to give a wide variety of responses, giving more scope to differences of ability and personality, which may be a gain from the sociological point of view, but results in lowered precision.

Hall, writing on two-alternative learning in inter-dependent dyads, stresses the need for a revision of basic theory. His experimental conditions encourage 'correct' action rather than the probability matching of responses. He does not attempt to patch over the old theory with new assumptions. Nevertheless, other contributors do attempt modifications of earlier models. Atkinson, for example, who is concerned with motivational complications produced by monetary reward, finds prediction from current learning theory to be defective, the subjects behaving more rationally than expected. He adapts theory by assuming a new factor of strength of conditioning. On the other hand, Estes, in investigating behaviour in two-person interactions with varying pay-offs, keeps to the old learning axioms. In his situations also, behaviour is more rational than the probability learning model would predict. He therefore modifies the 'responses rule', and introduces a 'scanning model', which incorporates a decision process leading to choice of the response expected to be most rewarding.

Later, the emphasis shifts to experiments in which the subjects have greater opportunity for self-programming, as in the articles by Smoke and Zajonc on the reliability of group judgements and decisions. The authors consider the problems raised by a task in which all members have an equal probability of being correct. They determine the relative efficiency of different ways of reaching this shared decision, including such structures as dictatorship, oligarchy, unanimity and quorum. Similarly, Iorge and Solomon present studies on group and individual behaviour in free recall verbal learning, and Restle on the speed and accuracy of cognitive achievements in small groups. Bush says that he realized ten years ago that there were no experiments and no data on two-person interactions, although he did discover an appreciable literature about two-person phenomena. For example, Parsons and other sociologists viewed two people as the simplest social system, and many psychologists wrote at length about parent-child relations, parent-therapist interactions, marital conflict, and so on. But no one did experiments. He therefore attempted to apply linear operator models, but time has, in his view, shown that this was not a very fruitful avenue of research. He considers that, although stochastic models for two-person interactions are interesting, we still lack any substantial body of data to serve as a basis for model making, and we are not even in agreement on what questions should be asked. However, Grisewell, in the Introduction, takes the view that, although the mathematics required seems to be greater than the data justifies, mathematical concepts derived from topology, formal logic and biology have been successfully formulated, and no doubt progress will slowly be made.

The verbal descriptions of the researches presented in this book are obscure, although the mathematical models are set out more rigidly than in many psychological articles. Specialization in psychology has now run amok, and there may well be as many as a hundred, or more, groups of psychologists who have little inkling of the import of one another's researches. One wonders how closely fellow contributors in this volume would understand one another's work. The elaboration of theory in this field appears to have outrun practical usefulness, but some good must surely come at last from so much laboratorial toil. F.W.WARBURTON

Approaches to the Study of Aphasia. By Charles E. Osgood and Murray S. Miron. Urbana: University of Illinois Press. 1963. Pp. vii+210. 40s.

This book is something of a *tour de force*, to whose challenge your reviewer is incapable of adequate riposte. It is the brilliant report, in the manner of the manifest dream content, of an inter-disciplinary (psychological, neurological, neuropsychiatric, linguistic, psycholinguistic, speech-pathological and what have you) research seminar and conference. A few participants met daily over a period of six weeks to discuss the problems of aphasia, among them Osgood and Miron, Schuell, Quadfasel and Weinreich. At the halfway point they were interrupted but not dammed up by the midstream conference when Noam (Electronics) Chomsky, Eisenson, Kurt Goldstein, Karl Pribram, Hans-Lukas Teuber, Wepman, and some twenty-five other distinguished inter-disciplinarians joined them for a week-end. 'Unless you have chaos within', said Nietzsche, 'you cannot create a dancing star'. Out of this coruscation of all the talents, Osgood and Miron, with heroic skill, have created a real book, a highly coherent exercise in documentation, which retains much of the vividness and vitality, the theoretical convergence and divergence, the momentary insight, the rigour of the research game, and the work-in-progress character of the day-to-day discussions. If only, it seems, somebody had not had a plane to catch—to Berkeley or Stanford, to Minneapolis, or Montreal, or Toronto—they might have been talking and thinking apparently indefatigably and to some purpose for months on end.

'Each of the disciplines represented by the conference focuses attention on a somewhat different set of dependent variables...The principal motivation for such a conference is the opportunity provided to check the validity of the theories of each of the separate disciplines against the observations made within the framework of related but separate disciplines.' With at least majority agreement the aphasic disorder is defined as 'a non-functional impairment in the reception, manipulation, and/or expression of symbolic content whose basis is to be found in organic damage to relatively central brain structures'.

In the light of this definition, much of the historical dogma since Broca, and of the broadening stream of research since Hughlings Jackson, comes under direct or indirect examination. The conference rightly found itself out of sympathy with a pragmatic conception of aphasia as being defined (like intelligence) by a particular kind of test performance, as well as with Goldstein's insistence—reminiscent of the far-off Wars of the Factorial Roses—that aphasia is a symptom of the single, unifying concept of abstraction. Whether the 'components' of aphasia are identifiable, they conclude, is an empirical question, but if factorial analysis can contribute to differential diagnosis or rehabilitation, then its revealed components are of value. The discussion of the problems of localization, central to neurological controversy, and particularly those of lateral cerebral dominance, are at once stimulating, balanced, and informative, and, in respect of Sperry's primate work, excitingly suggestive. On the issue of localization, the position reached by the seminarists was that it is more likely that *patterns of intersymptom correlation* are related to the locus of lesions, rather than that particular symptoms are *in isolation* correlated with specific lesions.

On the linguistic side, the profitable line of approach (Weinreich) would be to regard aphasic speech as an exotic language, hitherto unrecorded, and to proceed without preconception to its analysis. The psycho-linguistic approach starts from psychological theories of general behaviour; language behaviour (speech, reading, seminar discussion, writing reviews, reading reviews!) must be explained within this frame of reference. The contrast is pointed between the Skinner 'empty box' approach, and Osgood's explicit recognition of the problem of meaning and the need for (unobservable) intervening mechanisms.

The book presents an up-to-the minute conspectus of the various 'approaches' to the study of aphasia, and in doing so puts some current behavioural and neuro-psychological theory into a live clinical as well as academic context. The book must be regarded as 'required reading' for all those psychologists, and their advanced students, who have language and thinking, or aphasia, as a major field of professional or research interest. Psycholinguistic theory and research are accorded the fullest treatment, but at all points the discussion is highly relevant to psychological considerations, and rich in suggestions for specific lines of investigation.

The two valuable appendices should be noted: A, a very full bibliography of the literature of aphasia; and B, a useful psycholinguistic analysis, prepared by Dr Miron, of seventeen commonly used tests of aphasia, including those of Head, Goldstein and Scheerer, Weisenburg and McBride, and Halstead and Wepman.

GEORGE SETH

Neuro-Psychopharmacology. Proceedings of the Second Meeting of the Collegium Internationale Neuro-Psychopharmacologicum Basle. Edited by E. ROTHLIN. Amsterdam and New York: Elsevier. 1961. $24.00.

Specific and Non-Specific Factors in Psychopharmacology. Edited by MAX RINKEL. New York: Philosophical Library. 1963. $3.75.

The first-named book contains the proceedings of the Second Meeting of the Collegium Internationale Neuro-Psychopharmacologicum, which was held in Basle in 1960. There were five symposia: (1) The problem of antagonists to psychotropic drugs; (2) the effects of psychotropic drugs on conditioned responses in animals and man; (3) the influence of specific and non-specific factors on the clinical effects of psychotropic drugs; (4) measurement of changes in human behaviour under the effects of psychotropic drugs; (5) biochemical mechanisms related to the site of activity of psychotropic drugs. Each symposium was followed by individual papers on topics related to it. As at the previous meeting, two years before, there were contributors from many different countries and disciplines and a great variety of work is reported. Nearly every symposium contained something new and interesting (though sometimes one has to search for it), and also useful lists of references to the ever-growing literature of psychopharmacology. A symposium of particular interest to psychologists was that on the influence of the effects of so-called 'specific' and 'non-specific' factors on the clinical effects of psychotropic drugs. Basically, this is the problem of individual differences in reactions to drugs, and also the problem of why investigators often report contradictory therapeutic results. The answer is that one must take account not only of the typical or 'specific' effects of drugs on different kinds of behaviour, but also of the interactions between drugs and the circumstances in which they are administered: such factors as the clinical environment, the personality and past history of the patients, and so forth, which can markedly modify these typical effects. Although the influence of such factors has long been taken for granted it is only comparatively recently that they have been analysed systematically, both clinically and in laboratory experiments; the interactions can be very complex, but eventually it may be possible to make use of them with some confidence in practice. To the psychologist, also, the symposium on biochemical mechanisms related to the site of action of psychotropic drugs was in some ways among the most impressive, though the meaning of most of the relationships discussed is still, alas, as much a subject for discussion as are more psychological factors.

At the Third World Congress of Psychiatry in Montreal there was also a symposium on 'specific' and 'non-specific' factors. A different (almost) set of investigators gave fairly substantial papers on various aspects of the subject, including results of experiments on isolated tissues and with normal volunteers and on intact animals.

The general impression that both books leave with one is that research on drugs that have psychological effects remains as expanding, lively, important and incomplete as ever.

HANNAH STEINBERG

Alcoholism and Society. By MORRIS E. CHAFETZ and HAROLD W. DEMONE. London: Oxford University Press. 1962. Pp. xvi+319. 49*s*.

Alcoholic Addiction. By HOWARD JONES. London: Tavistock. 1963. Pp. x+209. 30*s*.

Chafetz and Demone, in a valuable addition to the literature on alcoholism, evaluate the growing evidence that alcoholism is in large measure socially determined, and that treatment calls for attention to cultural influences. One author is a psychiatrist at Massachusetts General Hospital, and the other a sociologist who was commissioner for alcoholism in Massachusetts. They evaluate the various theories about alcoholism in the light of the considerable range of alcoholism prevalence rates in different countries, and the differences between ethnic groups in the same country. In the individual alcoholic, multifactorial causation is held to operate; stress is laid on personality disturbance in early life, presence of a relative who used alcohol excessively, idiosyncrasy in physiological tissue response to alcohol, and psychological ambivalence regarding drinking.

While themselves adopting this comprehensive causal viewpoint, the authors give detailed attention to research which attempts to isolate operationally defined variables. Skolnick's study of the effect of religious affiliation is quoted (although criticized as concentrating on *one* factor): persons reared with an abstinence orientation, i.e. Methodists, are more likely to become addicted than those reared with a ritual orientation, i.e. Jews. The study by Straus and Bacon is described as an illustration of the search for indicators of pre-alcoholism; a young person guilty over drinking, e.g. a Mormon college student, is at greater risk of pathological drinking than his fellows who are not in opposition to parental values when they drink.

A chapter sets out the aims of Alcoholics Anonymous, developed from concepts which one of its pair of founders got from his association with the Oxford Group. (In his book Jones mentions that in A.A. extreme dependency on alcohol is replaced by dependency on the organization and adoption of rigid beliefs, some demonstrably untrue). A useful chapter surveys the diversity in government programmes in the United States. Although thirty-seven states have legislated for alcoholism services, administrators are uncertain whether to call the condition a disease, a sickness, or a public health problem; alcoholics are referred to sometimes as 'sufferers' and sometimes as 'victims'; states vary in assigning the alcoholism treatment services to mental health, public health or social welfare departments.

To improve treatment results and guide administrative efforts, the lax definitions, conflicting aetiological views and uncertain treatment approaches current at present need to be evaluated critically. Clinical criteria have yet to be agreed upon. The authors' own distinction between 'neurotic' drinkers who had integrated personalities before starting to drink, and 'addicts' who were always abnormal, is not acceptable on clinical grounds. Jellinek, who in 1952 set out the classical four phases in the development of alcoholism, is in 1962 concerned to subdivide alcoholism into five distinct types. More precise diagnosis may lead to more effective treatment;

the study by Wallerstein at the Menninger Clinic suggests that 'compulsive alcoholics' do best with Antabuse, depressed alcoholics respond to induced vomiting in aversion treatment, while passive-dependent alcoholics are helped by group hypnotherapy.

A series of eleven brief case-histories is presented in an appendix. In the text, clinical beliefs sometimes are expressed which are unverifiable or metaphorical. Depression is said to originate from loss of someone with whom one can relate emotionally in early life; alcohol is ingested to fill an ocean of emptiness; the alcoholic strives to adapt to life by seeking reunion with a symbolic all-giving mother figure.

Similar formulations are found helpful by Howard Jones, who writes, for example, that promiscuity in the alcoholic represents a ceaseless quest throughout life for his mother. *Alcoholic Addiction* describes an investigation of seventy-two subjects, half in an alcoholism treatment clinic and half in the Toronto Prison (convicted inebriates who had been sentenced for intoxication at least three times in the previous year). The research method adopted did not call for the author to remain unaware when typing the subjects which of them were clinic patients and which jail inebriates, nor was a reliability check carried out to see if another judge would assign subjects to similar categories, relating to individual differences inferred from protocols.

Three types of subject were distinguished, each with a characteristic state of mind: (1) The *maternal dependent* type, in whom early dependence on the mother is later transferred to the wife (18 subjects); (2) The *socially insecure* type, with inferiority complex, shy and insecure, and socially-dependent subtypes (30 subjects); (3) The *narcosis* type, with avoidance of unpleasant situations, alcohol being used to escape stressful demands (15 subjects). The clinic patients are mainly types 1 and 3; the jail subjects are predominantly type 2.

While the author concedes that the convicted inebriates suffer from a different social disease, he does not question his decision to combine the discrepant samples. He disclaims that his chief aim is to carry out a comparison between jail inebriates and clinic alcoholics. However, his pooling of the two samples obfuscates an issue which again emerges clearly in the subsequent analysis. The distinctions present between clinic and jail subjects may be the main importance of the study to many readers.

Jail subjects were older than clinic alcoholics; their educational level was lower; they were more than twice as often unemployed, their jobs were unskilled and these were changed more often; only half had married and hardly any of the marriages had lasted; they were most often Catholics; they were sceptical about therapy. Jail subjects tend to be bout drinkers and clinic subjects continuous drinkers.

H. J. WALTON

Sleeping and Waking. By IAN OSWALD. Amsterdam and New York: Elsevier. 1962. Pp. ix + 232. 45*s*.

The publication of a major monograph on this subject is naturally an occasion for hopeful anticipation, since there has been nothing of the kind since Kleitman's classical work published in 1937. Indeed Dr Oswald states that he hesitated to write his book because of the standard set by Kleitman. It is fortunate that he overcame any such hesitancy. The range of the book is extremely wide, including not simply an account of the physiological and psychological findings on sleep and arousal mechanisms, but taking in modern work on vigilance, mechanisms of attention, conditioning and behaviour therapy, hypnosis, and drugs. It certainly contains statements with which people will want to disagree, although few with which many will be in disagreement; for it is not merely a compendium of attested facts, but has the addition of a stimulating and constructive statement of the author's interpretation of his own and other people's work in this field (including one mis-ascribed reference).

Dr Oswald's own work is both fascinating and impressive, and is remarkable for the ingenuity with which he manages to apply experimental method to problems which have often seemed among the most intractable in psychology. It is matched by a high standard of writing, which makes it a greater pleasure to read than most scientific books, being agreeably spiced with well-chosen anecdotal evidence to enliven what is perhaps more commonly thought to be 'respectable' evidence.

The standard of production is high and the book deserves a place beside Kleitman as a classical contribution to the literature.

NEVILLE MORAY

Progress in Clinical Psychology. Vol. v. Edited by LAWRENCE E. ABT and BERNARD F. RIESS. New York: Grune and Stratton. 1963. Pp. xi + 209. $7.50.

With the publication of this volume a decade of progress has been reviewed in this series, which, the Editorial Board announces, has successfully 'come of age'. The series, in the reviewer's opinion, has never been noteworthy as a chronicle of advances in clinical psychology and the current volume creates the impression that American clinical psychology has meandered to its present position. The fault lies with the Editorial Board and its policy and not with the individual contributions. Had the editors taken their duties as involving more than compilation they could have given a better sense of direction to the discipline's progress.

In the current volume, the best of the chapters is by Robert and Selina Morrow on the psychological aspects of physical disability. Liverant contributes a thoughtful review of Learning Theory and Clinical Psychology, which somehow lacks the polemic force that we in this country have come to expect from such a title. A Russian contribution on cerebral palsied children is of interest but is out of place in this volume. Liebermann's discussion on Forensic Psychology will appeal to those who are awakening to such issues in this country. There are two chapters on Dreams and one each on progress in Adlerian theory and in Jungian theory respectively. The remaining of the thirteen chapters deal with aspects of psychotherapy, including a discussion of the clinical psychologist's role in this field which is of purely American interest.

Clinical psychologists in this country might find something of interest in this volume. However, it should be given a low priority for personal purchase or for recommendation to the hospital librarian.

R. M. MOWBRAY

Biochemistry of Mental Disease. By THEODORE L. SOURKES. London: Harper and Row. 1962. Pp. xxiii + 402. 96*s*.

Most clinical psychiatrists and psychologists find themselves at a loss when their biochemical colleagues talk about 5-HT, OMT, DPT and other strange groups of letters, which have a vague resemblance to the abbreviations of the various offshoots of the United Nations. Now with the publication of this excellent review of the biochemistry of mental disorders there is no need for such ignorance. However, this does not mean that the uninitiated reader will find this book easy to read. Although Dr Sourkes has written a very useful introduction to a difficult subject, his book has a few blemishes. He has, unfortunately, no knowledge of psychiatry, so that there are inadequate descriptions of psychiatric syndromes associated with biochemical disorders. Another irritating feature is the method of cross-reference to equations, which forces the reader to find the relevant chapter in the table of contents and then thumb through the chapter until he finds the equation. Finally, the book is written in rather quaint English, which leads one to suppose that English is not the author's native tongue. Perhaps the most memorable sentence in the book is 'Riboflavin-deficient swine assume an uneconomical appearance and fail to gain weight'.

However, despite all these minor criticisms, this book can be unreservedly recommended to psychologists, psychiatrists and others who are interested in the biochemistry of mental illnesses.

FRANK FISH

Recovery from Early Blindness. By R. L. GREGORY and J. G. WALLACE. Cambridge: Heffer. 1963. (E.P.S. Monograph, no. 2.). Pp. vi + 46. 20*s*.

This is an interesting but rather irritating book. It is the report of an investigation made by two Cambridge psychologists into the reactions of a man (S.B.) of 52 years who recovered his sight after having been blind for 51 years. It is not a planned piece of research as they did not hear of the case until S.B. was already in hospital, having had his first operation. Unfortunately, this is obvious in the lack of systematic testing and the rather haphazard way in which actual reactions were elicited. It meant also, of course, that they had no personal knowledge of the subject's responses during the time he was blind. They took considerable care, however, to get reports from as many people as possible concerning his developmental period and his way of life since maturity. There is one further drawback: neither author had had any experience of work with blind people and they appear a little naïve; e.g. they were surprised that a blind man could tell the time by his own watch.

Nevertheless, there is much of very real interest in the monograph particularly as there are so few modern records of this type of experience. It is significant that S.B., throughout his blind life an independent, aggressive, active man, became so dispirited and unhappy after gaining his sight. 'We felt he had lost more than he had gained by recovering his sight.'

There is an interesting discussion of the meaning and significance to various types of people of the word 'blind' at the beginning of the book and a critical discussion of the relevance of this man's experience to the theory of perception at the end. Hebb's work in this field and von Senden's work in the blind field are discussed. There is no Index.

WINIFRED LANGAN

Shakespeare's Melancholics. By W. I. D. SCOTT. London: Mills and Boon. 1962. Pp. 192. 21*s*.

Sceptics have maintained that it is idle—or at best an academic exercise—to interpret literary creation in terms of psychological or psychiatric categories. Nevertheless, readers who have had experience of both disciplines (literature and psychology) will recognize that each can throw light on the other, and that considerations of scientific validity need not arise. Dr Scott, who is an actor and producer as well as a psychiatrist, has addressed himself primarily to the non-psychologist, but even psychologists—or perhaps they more than others—will find that his study helps even more towards an understanding of what is usually referred to in the book as morbid psychology, than of the characters discussed. 'Melancholics' in Dr Scott's title is used in the broad, Elizabethan sense, rather than in its narrower present-day technical connotation. Eight characters ranging, in length of treatment, from Hamlet (35 pp.) to Don John (*Much Ado*, 3 pp.) are dealt with, and among them they cover nearly the whole gamut of psychopathology. The author's central position is that 'Shakespeare assimilated the persons in his plays to types which he observed around him, and that, intuitively, he let them control his plots according to a complexity in their nature of which he himself was only partly aware' (p. 43). On the basis of this premise he examines his texts with great thoroughness in order, as he says, 'to fulfil the aim of drawing inferences from Shakespeare's own words rather than from unsupported theory' (p. 73). The result makes fascinating and—in the widest sense—profitable reading.

B. SEMEONOFF

Brit. J. Psychol. (1963), 54, 4, *pp.* 385–386
Printed in Great Britain

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(Inclusion in this list does not necessarily preclude later review.)

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Values and Intentions. By J. N. Findlay. London: Allen and Unwin. 1961. Pp. 442. 55*s.*

Sign and Design. The Psychogenetic Source of the Alphabet. By Alfred Kallir. London: James Clarke. 1961. Pp. vi + 354. 55*s.*

The Psychology of Driving. Factors of Traffic Enforcement. By A. R. Lauer. Oxford: Blackwell. 1961. Pp. xxvii + 324. 84*s.*

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